# REPORT

OF THE

# FIFTH MEETING

OF THE

# BRITISH ASSOCIATION

FOR THE

# ADVANCEMENT OF SCIENCE;

HELD AT DUBLIN IN 1835.

LONDON:

JOHN MURRAY, ALBEMARLE STREET.

1836.

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# CONTENTS.

## ERRATUM.

In Mr. Whewell's Report on Heat, Electricity, and Magnetism, it is stated that a remark of Sir John Herschel's respecting the temperature of the celestial spaces, is contained in a memoir not yet published. This is a mistake, the memoir referred to having been published in the *Geological Transactions*, vol. v. part ii.

# OBJECTS AND RULES

# THE ASSOCIATION.

## OBJECTS.

The Association contemplates no interference with the ground occupied by other Institutions. Its objects are,—To give a stronger impulse and a more systematic direction to scientific inquiry,—to promote the intercourse of those who cultivate Science in different parts of the British Empire, with one another, and with foreign philosophers,—to obtain a more general attention to the objects of Science, and a removal of any disadvantages of a public kind, which impede its progress.

## RULES.

### MEMBERS.

All Persons who have attended the first Meeting shall be entitled to become Members of the Association, upon subscribing an obligation to conform to its Rules.

The Fellows and Members of Chartered Literary and Philosophical Societies publishing Transactions, in the British Empire, shall be entitled, in like manner, to become Members of the Association.

The Officers and Members of the Councils, or managing Committees, of Philosophical Institutions shall be entitled, in like manner, to become Members of the Association.

All Members of a Philosophical Institution recommended by its Council or Managing Committee, shall be entitled, in like manner, to become Members of the Association.

Persons not belonging to such Institutions shall be elected by the General Committee or Council, to become Members of the Association, subject to the approval of a General Meeting.

## SUBSCRIPTIONS.

The amount of the Annual Subscription shall be One Pound, to be paid in advance upon admission; and the amount of the *composition in lieu thereof, Five Pounds.*

Subscriptions shall be received by the Treasurer or Secretaries.

If the annual subscription of any Member shall have been in arrear for two years, and shall not be paid on proper notice, he shall cease to be a member; but it shall be in the power of the Committee or Council to reinstate him, on payment of arrears.

## MEETINGS.

The Association shall meet annually, for one week, or longer. The place of each Meeting shall be appointed by the General Committee at the previous Meeting; and the Arrangements for it shall be entrusted to the Officers of the Association.

## GENERAL COMMITTEE.

The General Committee shall sit during the time of the Meeting, or longer, to transact the business of the Association. It shall consist of all Members present, who have communicated any scientific Paper to a Philosophical Society, which Paper has been printed in its Transactions, or with its concurrence.

Members of Philosophical Institutions, being Members of this Association, who may be sent as Deputies to any Meeting of the Association, shall be Members of the Committee for that Meeting, the number being limited to two from each Institution.

## SECTIONAL COMMITTEES.

The General Committee shall appoint, at each Meeting, Committees, consisting severally of the Members most conversant with the several branches of Science, to advise together for the advancement thereof.

The Committees shall report what subjects of investigation they would particularly recommend to be prosecuted during the ensuing year, and brought under consideration at the next Meeting.

The Committees shall recommend Reports on the state and progress of particular Sciences, to be drawn up from time to time by competent persons, for the information of the Annual Meetings.

## COMMITTEE OF RECOMMENDATIONS.

The General Committee shall appoint at each Meeting a Committee, which shall receive and consider the Recommendations of the Sectional Committees, and report to the General Committee the measures which they would advise to be adopted for the advancement of science.

## LOCAL COMMITTEES.

Local Committees shall be formed by the Officers of the Association to assist in making arrangements for the Meetings.

Committees shall have the power of adding to their numbers those Members of the Association whose assistance they may desire.

## OFFICERS.

A President, two Vice-Presidents, two or more Secretaries, and a Treasurer, shall be annually appointed by the General Committee.

## COUNCIL.

In the intervals of the Meetings the affairs of the Association shall be managed by a Council, appointed by the General Committee.

## PAPERS AND COMMUNICATIONS.

The Author of any paper or communication shall be at liberty to reserve his right of property therein.

## ACCOUNTS.

The Accounts of the Association shall be audited annually, by Auditors appointed by the Meeting.

# OFFICERS AND COUNCIL, 1835-6.

*Trustees (permanent).*—Charles Babbage, Esq. R. I. Murchison, Esq. John Taylor, Esq.

*President.*—Rev. Bartholomew Lloyd, D.D. Provost of Trinity College, Dublin.

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James Apjohn, M.D. Francis Barker, M.D. Richard Griffith, Esq. John Hart, M.D. Robert Hutton, Esq. Samuel Litton, M.D. Mountifort Longfield, LL.D. Captain Portlock. Rev. George S. Smith. Isaac Weld, Esq. And the Officers of the Association residing in Dublin.

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Richard Cane, Esq. A. Carmichael, Esq. George Carr, Esq. J. Sisson Cooper, Esq. Hon. Thomas Barnwall. Loftus Bland, Esq. Robert Borrowes, Esq. E. Borough, Esq. Charles Brown, jun., Esq. J. W. Cusack, M.D. Robert Graves, M.D. Charles W. Hamilton, Esq. J. R. Corballis, Esq. W. Edington, Esq. J. Pim, jun., Esq. Cornelius Sullivan, Esq. Captain Hooke. Arthur Hume, Esq. Henry Kemmis, Esq. Hon. Edward Lawless. B. C. Lloyd, Esq. Sir Charles Morgan. Hon. Sir Francis Stanhope. W. Stokes, M.D.

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### SECTION F.—STATISTICS.

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# BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

## TREASURER'S ACCOUNT from 31st July 1834 to 30th June 1835.

### RECEIPTS.

| | £. | s. | d. |
|---|---|---|---|
| Balance in hand from last year's Account | 291 | 16 | 4 |
| Compositions from 101 Members | 491 | 0 | 0 |
| Subscriptions 1834, 1135 do. | 1135 | 1 | 0 |
| Ditto 1835, 7 do. | 7 | 0 | 0 |
| Arrears 1833, 29 do. | 29 | 1 | 0 |
| Dividend on 1700*l.*, in 3 per cent. Consols, 6 months to January last | 25 | 10 | 0 |
| Interest on Cash in Edinburgh Bankers to January last | 8 | 7 | 1 |
| Received on account of Sale of Reports, 1st volume | 50 | 18 | 1 |
| Ditto ditto 2nd volume | 207 | 0 | 0 |
| Ditto ditto Lithographs | 45 | 8 | 0 |
| | £2291 | 1 | 6 |

G. B. GREENOUGH,
FRANCIS BAILY, } *Auditors.*

### PAYMENTS.

| | £. | s. | d. | £. | s. | d. |
|---|---|---|---|---|---|---|
| Expenses of Meeting at Edinburgh | | | | 245 | 10 | 8 |
| Disbursements by Local Treasurers | | | | 78 | 6 | 3 |
| Purchase of £800 in 3 per cent. Consols | | | | 739 | 0 | 0 |
| Salaries to Assistant Secretary and Accountant to Christmas last | | | | 145 | 0 | 0 |
| Paid the "Committee for the Discussion of Tide Observations" | | | | 62 | 0 | 0 |
| Paid the Committee on British and Foreign Ichthyology | | | | 105 | 0 | 0 |
| Paid to S. D. Broughton, for Experiments on the Cerebral Nerves | | | | 25 | 0 | 0 |
| Paid Sundry Expenses on Reports, 1st volume | | | | 34 | 2 | 1 |
| Paid Richard Taylor Printing 2nd edition of Reports, 1st volume, 750 Copies | | | | 207 | 17 | 6 |
| Paid Richard Taylor Printing Reports, 2nd volume, 1500 Copies | 358 | 4 | 11 | | | |
| Less charged on Account last year | 250 | 0 | 0 | | | |
| | | | | 108 | 4 | 11 |
| Expenses on publishing Reports, 2nd volume | | | | 28 | 4 | 11 |
| Expenses on Lithographs | | | | 2 | 18 | 11 |
| June 30, Balance in the Banker's hands | 485 | 8 | 5 | | | |
| Ditto Treasurer's | 12 | 1 | 10 | | | |
| Ditto Local Treasurers' | 12 | 6 | 0 | | | |
| | | | | 509 | 16 | 3 |
| | | | | £2291 | 1 | 6 |

[Signed,] JOHN TAYLOR, *Treasurer.*

# DESIDERATA,

## &c.

The following Reports on the progress and desiderata of different branches of science have been drawn up at the request of the Association, and printed in its Transactions.

On the progress of Astronomy during the present century, by G. B. Airy, M.A., Astronomer Royal.

On the state of our knowledge respecting Tides, by J. W. Lubbock, M.A., Vice-President of the Royal Society.

On the recent progress and present state of Meteorology, by James D. Forbes, F.R.S., Professor of Natural Philosophy, Edinburgh.

On the present state of our knowledge of the Science of Radiant Heat, by the Rev. Baden Powell, M.A., F.R.S., Savilian Professor of Geometry, Oxford.

On Thermo-electricity, by the Rev. James Cumming, M.A., F.R.S., Professor of Chemistry, Cambridge.

On the recent progress of Optics, by Sir David Brewster, K.C.G., LL.D., F.R.S., &c.

On the recent progress and present state of Mineralogy, by the Rev. William Whewell, M.A., F.R.S.

On the progress, actual state, and ulterior prospects of Geology, by the Rev. William Conybeare, M.A., F.R.S., V.P.G.S., &c.

On the recent progress and present state of Chemical Science, by James F. W. Johnston, A.M., Professor of Chemistry, Durham.

On the application of Philological and Physical researches to the History of the Human Species, by J. C. Prichard, M.D., F.R.S., &c.

On the advances which have recently been made in certain branches of Analysis, by the Rev. G. Peacock, M.A., F.R.S., &c.

On the present state of the Analytical Theory of Hydrostatics and Hydrodynamics, by the Rev. John Challis, M.A., F.R.S., &c.

On the state of our knowledge of Hydraulics, considered as a branch of Engineering, by George Rennie, F.R.S., &c. (Parts I. and II.)

On the state of our knowledge respecting the Magnetism of the Earth, by S. H. Christie, M.A., F.R.S., Professor of Mathematics, Woolwich.

On the state of our knowledge of the Strength of Materials, by Peter Barlow, F.R.S.

On the state of our knowledge respecting Mineral Veins, by John Taylor, F.R.S., Treasurer G.S., &c.

On the state of the Physiology of the Nervous System, by William Charles Henry, M.D.

On the recent progress of Physiological Botany, by John Lindley, F.R.S., Professor of Botany in the University of London.

On the Geology of North America, by H. D. Rogers, F.G.S.

On the philosophy of Contagion, by Wm. Henry, M.D., F.R.S.

On the state of Physiological knowledge, by the Rev. William Clark, M.D., F.G.S., Professor of Anatomy, Cambridge.

On the state and progress of Zoology, by the Rev. Leonard Jenyns, M.A., F.L.S., &c.

On the theories of Capillary Attraction, and of the Propagation of Sound as affected by the development of Heat, by the Rev. John Challis, M.A., F.R.S., &c.

On the state of the science of Physical Optics, by the Rev. H. Lloyd, M.A., Professor of Natural Philosophy, Dublin.

On the state of our knowledge respecting the application of Mathematical and Dynamical principles to Magnetism, Electricity, Heat, &c., by the Rev. William Whewell, M.A., F.R.S.

On Hansteen's researches in Magnetism, by Captain Sabine, F.R.S.

On the state of Mathematical and Physical Science in Belgium, by M. Quetelet, Director of the Observatory, Brussels.

The following Reports and Continuations of Reports have been undertaken to be drawn up at the request of the Association:

1. On the progress of Electro-chemistry and Electro-magnetism, so far as regards the Experimental part of the subject, by P. M. Roget, M.D., Sec. R.S.

2. On the Connexion of Electricity and Magnetism, by S. H. Christie, F.R.S., &c.

3. On the state of knowledge on the Phænomena of Sound, by the Rev. Robert Willis, M.A., F.R.S.

4. On the state of our knowledge respecting the relative level of Land and Sea, and the waste and extension of the land on the east coast of England, by R. Stevenson, Engineer to the Northern Lighthouses, Edinburgh.

5. On the Zoology of North America, by J. Richardson, M.D., F.R.S.

6. On the Botany of North America, by Jacob Greene, M.D., and Sir W. J. Hooker, M.D., Professor of Botany, Glasgow.

7. On the Geographical distribution of Insects, and particularly the order Coleoptera, by J. Wilson, F.R.S.E.

8. On the influence of Climate upon Vegetation, by the Rev. J. S. Henslow, M.A., F.L.S., Professor of Botany, Cambridge.

9. On circumstances in Vegetation influencing the Medicinal virtues of Plants, by R. Christison, M.D., &c.

10. On the Vegetation of Ireland and Scotland, by Mr. Mackay.

11. On Mineral Waters, by Professor Daubeny.

12. On Salts, by Professor Graham.

13. On the progress of Medical Science in Germany, by Dr. Graves.

14. On the Differential and Integral Calculus, by the Rev. G. Peacock, M.A., F.R.S.

15. On the Theories of Capillary Attraction, &c., by the Rev. J. Challis, M.A., F.R.S.

16. On the present state of our knowledge of the Phænomena of Terrestrial Magnetism, by Captain Sabine, F.R.S.

17. On the Geology of North America, by H. D. Rogers, F.G.S., Professor of Geology, Philadelphia.

## ASTRONOMY.

*Resolutions of the Committee.*

1. The Committee for Mathematical and Physical Science having stated that it would tend much to the advancement of astronomy, and the art of navigation, if the observations of the sun, moon, and planets, made by Bradley, Maskelyne, and Pond, were reduced,—

It was resolved by the General Committee, that a representation to this effect from the British Association be submitted to Government, in the hope that public provision might be made for the accomplishment of this great national object, and that a deputation, consisting of Professor Airy, Mr. Baily, Mr. D. Gilbert, and Sir John Herschel, be appointed to confer with the Lords of the Treasury on the subject*.

2. That application be made to the French Government on the part of the Association, for the purpose of obtaining a reduction of the astronomical observations made at the Ecole Militaire, and published in the *Histoire Céleste* and in the volumes of the Académie des Sciences for 1789, 1790; and that,

* The application was immediately complied with by the Government, and an advance of 500*l.* made by the Treasury; the reduction of the observations from the year 1750 to the present day is in progress.

provided the French Government agree to this proposition, the sum of 500*l.* be placed at the disposal of a Committee, to be hereafter nominated by the Council, for the purpose of procuring the duplicate reduction which is necessary to ensure accuracy *.

3. That the difference of meridians between the Observatories of Greenwich, Cambridge, Oxford, Edinburgh, Dublin, and Armagh, should be determined by means of chronometers, or by signals, or by both methods.

4. That it is desirable that the constant of lunar notation should be deduced from observations made with the mural circle at Greenwich, and that the sum of 100*l.* be appropriated to this purpose, under the direction of Sir T. Brisbane, Rev. Dr. Robinson, and Mr. Baily.

5. That it is desirable that the standard scale made some years ago by Mr. Troughton for the town of Aberdeen should be compared with the standard scale recently made for the Royal Astronomical Society, and that Mr. Baily be requested to make the requisite comparisons †.

*Desiderata noticed in Mr. Airy's Report, p.* 187.

1. Directions for placing a thermometer so as to indicate correctly the Temperature of the Air at the place of observation, for Refraction-corrections, the external and internal temperatures being supposed as nearly as possible equal.

2. Experimental Data for the Theory of Refraction—

   What is the law of the decrease of temperature, or of density, in ascending?

   How does this vary at different times?

   Can any means be contrived for indicating practically at different times the modulus of variation?

   Does the refractive power of air depend simply on its density, without regard to its temperature?

   Is it well established that the effects of moisture are almost insensible?

   Can any rule be given for estimating the effect of the difference of refraction in different azimuths, according to the form of the ground?

   When the atmospheric dispersion is considerable, what part of the spectrum is it best that Astronomers should agree to observe?

3. An investigation of the coefficient of Nutation from the Greenwich circle-observations.

* An application has been made through the Bureau des Longitudes.

† These comparisons have been made; see p. 91.

4. The Reduction of Bradley's and Maskelyne's Observations of the Sun and Planets, on a uniform plan.

5. Remeasurement* of the elongation of Jupiter's Satellites, to correct the estimate of the mass of Jupiter.

6. Separate investigations, from observations, of the diminution of the aphelion distance and perihelion distance of Encke's Comet, for the purpose of testing the truth of Encke's assumed law of density of the resisting medium.

7. Calculations of the perturbations of Biela's Comet for the interval between 1772 and 1806, and of those of the node and inclination from 1806 to 1826, for the purpose of ascertaining the identity of the comet of 1772, and examining whether this comet gives any indication of a resisting medium.

8. Verification of Burckhardt's Formulæ in the *Mém. de l'Inst.* for 1808, and extension of them to terms depending on the inclination.

9. Theory of the perturbations of Pallas, and of Encke's Comet.

## TIDES.

*Resolutions of the Committee.*

1. That a sum not exceeding 250*l.* be placed at the disposal of Mr. Lubbock for the discussion of observations of the Tides.

2. That the Association should endeavour to procure the general establishment of systematic Tide Observations along the coasts of Great Britain and Ireland, and that the standing Committee on Tides be requested to select such places† as may appear to them most important for this purpose; that the direction, and if possible the intensity, of the wind should be observed, as well as its critical changes after having set for some time in a particular direction; and that the altitude of the currents of air should also be, as far as possible, remarked.

## METEOROLOGY.

*Resolutions of the Committee.*

1. That a series of observations of the Thermometer during every hour of the day and night, be instituted at some military or naval station in the South of England.

* Prof. Airy himself has since given a determination of the mass of Jupiter.

† Directions for observing the Tides, extracted from Mr. Lubbock's Report, and Mr. Whewell's Memoranda, are inserted p. xxxiii.

1 *a*. That the sum of 50*l*. be applied to meet the expenses of the observations which, in pursuance of this recommendation, have been carried on for three years and a half by the Wardens and inferior officers in Plymouth Dockyard, under the directions of Mr. W. S. Harris*.

2. That a similar hourly register be recommended to be established under the superintendence of the Committee of the Association in India.

3. That the Committee in India be requested to institute such observations as may throw light on the horary oscillations of the Barometer near the equator.

4. That Mr. Phillips, and Mr. William Gray, jun., be requested to undertake a series of observations on the comparative quantities of rain falling on the top of the great tower of York Minster, and on the ground near its base; and that similar observations be instituted at other places †.

5. That persons travelling on mountains, or ascending in balloons, should observe the state of the Thermometer, and of the dew-point Hygrometer, below, in, and above, the clouds, and determine how the different kinds of clouds differ in these respects.

6. That the decrease of Temperature at increasing heights in the atmosphere, should be investigated by continued observations at stated hours, and known heights. The hours of $9\frac{1}{4}$ A.M., and $8\frac{1}{2}$ P.M., as giving nearly the mean temperature of the year, are suggested for the purpose.

7. That the temperature of Springs should be observed at different heights above the mean level of the sea, and at different depths below the surface of the earth, and compared with the mean temperature of the air and the ground ‡; and that notice be given that any persons who may be able to obtain the temperature of the air, water, and rock in mines and borings of known depth, or the indications of thermometers sunk to differ-

* For the report of these observations, and those made under the direction of the late Mr. Harvey, see *Reports*, vol. iv. pp. 171 and 181.

† The observations at York were made at three adjacent stations of known height, with gauges made on the same mould, and measured by one graduated glass vessel; they were continued from the 1st of February, 1832, to the 1st of February, 1835. From the results it has been inferred by Professor Phillips that the diminution in the quantity of rain at the higher stations has a certain constant dependence on the height of the station, and on the condition of the air as to temperature and moisture in the different periods of the year. For the further elucidation of this subject, it is desirable that experiments upon the same plan should be tried in other situations, and especially where the climate is of a different character from that of York; in the humid atmosphere of Cornwall, for example, and in the drier air of the midland counties.

‡ The height of the springs may be determined with sufficient accuracy by a common portable barometer.

ent depths, in different kinds of soil and in different parts of the earth, are requested to make known their names and the places where they have this opportunity, to the Secretary of the Meteorological Committee*.

7 *a*. That the Meteorological Committee be requested to give instructions, and to make arrangements on the subjects of the Experiments recommended in the above resolution, and that 100*l*. be placed at their disposal.

8. That the Committee be further requested to draw up an account of the best form of Meteorological instruments;—to prepare standard instruments, both as a means of comparison and of construction of similar instruments, for those Members of the Association who may wish to purchase Instruments constructed by these standards;—to draw up Forms, Register Tables and Abstracts, with directions for the best times, places, and methods of observing and registering;—and that a further sum of 100*l*. be placed at their disposal, for the above purpose and others connected with the advancement of Meteorology.

That series of comparative experiments should be made on the temperature of the dew-point, and the indications of the wet-bulb Hygrometer, and that the theory of this instrument should be further investigated †.

9. That instructions for observing Auroral Phænomena and Falling Stars be drawn up, with a view to the procurement of corresponding observations in every part of the kingdom ‡.

### *Desiderata noticed in Prof. Forbes's Report.*

Verification of Mr. Dalton's theory of the constitution of the atmosphere, by direct experiment. (*Reports*, vol. i. p. 206. and *Phil. Trans.* 1826.)

Experiments in various latitudes upon the temperature of the earth at moderate depths, by means of thermometers with long tubes; with a view to determine the position of the "invariable stratum," where external causes cease to produce any effect. (*Reports*, vol. i. p. 221.)

Experiments on the solar and terrestrial radiation. (*Reports*, vol. i. p. 222.)

* The members of the Committee are Dr. Apjohn, Prof. Forbes, Mr. W. S. Harris, Mr. J. Hudson, Professor Phillips, Professor Powell, Colonel Sykes, Mr. John Taylor (Secretary). The Committee has had instruments constructed suitable for experiments on temperature in mines, &c., and some of them are now in use at selected points.

† For an investigation of the theory of this instrument, see Dr. Apjohn's paper, and Dr. Hudson's remarks in *Phil. Mag.*, 1835–6.

‡ An abstract of the directions which have been drawn up by the Committee is given page xxxv.

Observations on the horary oscillations of the barometer at considerable heights above the sea. This more particularly applies to places near the equator *.

Additional observations to determine what is the influence of the moon on the height of the barometer. (*Reports*, vol. i. p. 234. See also *Arago, Annuaire for* 1833.)

The application of the hygrometric correction to the barometric formulæ for heights. (*Reports*, vol. i. p. 254.)

Observations on the phænomena of wind at two stations at considerably different elevations. (*Reports*, vol. i. p. 249.) The direction of the wind should be noted in *degrees*, beginning from the south and proceeding by the west.

Magnetical observations, regularly conducted, especially with a view to auroral phænomena.

## MAGNETISM.

### *Resolutions of the Committee.*

1. That a series of observations upon the intensity of Terrestrial Magnetism be executed in various parts of the kingdom, similar to those which have been carried on in Scotland by Mr. Dunlop †.

2. That observations should be made in various places with the Dipping-needle, in order to reduce the horizontal to the true magnetic intensity.

3. That it be represented to the Government of this country that it would be of great service to Science if Magnetical and Meteorological Observatories were established in several parts of the earth, furnished with proper Instruments, well conducted on uniform principles, and if provision were made for careful and continued observations at those places; that in Great Britain and its colonies there are points favourable for such observations; and that it is more desirable that the British nation should take a part in carrying them on, since a system of similar observations has begun to be established in France and its dependencies.

* Those who may possess such observations, continued for one or more weeks, with observations of the temperatures of the mercury and of the air, and *the probable corresponding temperatures of the air at the level of the sea*, are requested to transmit them to Professor Forbes, Edinburgh. The local position of the point of observation should also be noticed.

† See Dr. Traill's experiments, *Reports of the Association*, vol. i. p. 557: and those of Professor Lloyd, vol. iv. p. 117.

That Mr. Baily, Mr. Davies Gilbert, Mr. Lubbock, and the Rev. G. Peacock, be a Committee to make the required representation to the Government, and to solicit the cooperation of the French Institute.

4. That the East India Company be requested to further the same objects, especially at their establishment at Madras.

5. That M. Arago be respectfully requested to publish, and to have reduced, his valuable and extensive collection of Magnetical Observations made at the Observatory at Paris.

6. That a representation be made to Government of the importance of sending an expedition into the Antarctic regions, for the purpose of making observations and discoveries in various branches of Science, as Geography, Hydrography, Natural History, and especially Magnetism, with a view to determine precisely the place of the Southern Magnetic Pole or Poles, and the direction and inclination of the magnetic force in those regions.

7. That a further examination of the Electro-magnetic condition of mineral veins be recommended.

*Desiderata noticed in Professor Christie's Report.*

1. A regular series of observations conducted in this country on the diurnal variation of the needle.

2. To ascertain how far the method of 'torsion' is applicable to determine the diurnal variation of the horizontal magnetic force. (*Reports*, vol. ii. p. 119.)

3. To ascertain experimentally the direction of electrical currents occasioned in a large sphere of copper filled with bismuth, heated at the equator, and cooled at the poles, with a view to the theory of the origin of the earth's magnetic polarity. (*Reports*, vol. ii. p. 132.)

## RADIANT HEAT.

*Desiderata reported by Professor Powell.*

1. The accurate verification of Sir J. Leslie's observation that the *focus* for *simple heat*, in concave reflectors, is nearer to the reflector than that for light. (*Leslie on Heat*, p. 14.)

2. The proportion of heat reflected at different incidences.

3. Whether radiation takes place in an *absolute* vacuum.

4. Whether heat is *radiated* from hot bodies *in* liquid media, and whether it is *reflected*, &c., and has the same relations to *surfaces* as in air.

5. *Accurate* determinations of the *conducting* powers of different solids, liquids, and gases; distinguishing from the effects of radiation (if any) in or through them; and examining the modification (if any) which the ratios of the conducting powers undergo from difference of thickness, especially when the thickness is extremely small*.

*Desiderata mentioned in Mr. Whewell's Report on the Mathematical Theories of Electricity, Magnetism, and Heat.*

1. A comparison of good recent measures of the statical forces in electrical experiments (those of Mr. Harris and any others) with the Coulombian theory.
2. A determination of the degree of exactness attained and attainable by means of Mr. Barlow's correcting-plate.
3. Measures of the rate of increase of the temperature of the earth's mass in descending (both in given places and on the average), to compare with similar observations at a future period.
4. A comparison of the observed law of temperatures, as depending on the latitude, with Fourier's formulæ.
5. Treatises in which the results of the theories here spoken of (Coulomb and Poisson's theories of electricity and magnetism, and Fourier's theory of heat,) shall be presented in a manner sufficiently elementary to be accessible to mathematical readers of common attainments, as, for instance, the readers of Newton.

## OPTICS.

*Resolutions of the Committee.*

1. That a sum not exceeding 80*l.* be appropriated to the construction of a Telescopic Lens, or Lenses, out of rock salt, under the direction of Sir David Brewster.
2. That a sum not exceeding 15*l.* be placed at the disposal of Professor Powell, for carrying on Experiments on the refractive indices of different media on the principle of Fraunhofer, as recommended in the reports of the Association.
3. That 30*l.* be placed at the disposal of Prof. Wheatstone, for procuring a Theodolite and Prism to prosecute his researches on the prismatic examination of the Electro-Magnetic spark.

* See for observations on Radiant Heat Dr. Hudson's paper in the Fourth volume of the *Association's Reports*, p. 163.

*Desiderata noticed in Sir David Brewster's Report.*

The determination of various *constants*—namely,

1. The refractive indices of the two pencils in all crystallized bodies, measured in reference to definite points of the spectrum.
2. The angles at which light is polarized by reflection from crystallized and uncrystallized surfaces.
3. The inclination of the resultant axes of crystals having double refraction, for different rays of the spectrum.
4. The dimensions of the ellipse which regulates the polarization of metals and their alloys.
5. The circularly polarizing forces of fluids and solutions.
6. The refractive and dispersive powers of ordinary solid and fluid bodies, measured according to the method of Fraunhofer.
7. Experimental determination of the effects of the absorption of light by gases upon the light of the fixed stars. (vol. i. p. 322.)

*Desiderata noticed in Professor Lloyd's Report.*

1. Measurement of the refractive indices corresponding to the seven principal fixed lines of the spectrum in various singly refracting substances, according to the method of Fraunhofer.
2. Similar determination of the principal refractive indices of crystals, in continuation of the researches of M. Rudberg.
3. Experimental examination of Fresnel's theory of double refraction, in biaxal crystals.
4. Comparison of Fresnel's formulæ for the intensity of reflected and refracted light, with observation.
5. Theory of reflexion at the surfaces of crystallized media and metals, according to the principles of the wave theory.
6. Explanation of the peculiar laws of double refraction and polarization in rock crystal, according to the same theory.
7. Physical account of the phænomenon of circular polarization in liquids.
8. Physical theory of absorption.

## CHEMISTRY.

*Resolutions of the Committee.*

1. That Dr. Dalton and Dr. Prout be requested to institute experiments on the specific gravities of Oxygen, Hydrogen, and Carbonic Acid, and that a sum not exceeding 50*l.* be appropriated to defray the expense of any apparatus which may be required.

2. That British chemists be invited to make experiments for removing the doubts respecting the proportions of Oxygen, Azote, &c., in the Atmosphere; for determining the proportions of Azote and Oxygen in Nitrous Gas and Nitrous Oxide; and for more accurately investigating the specific gravity of the compound gases in general.

3. That the sum of 25*l.* be placed at the disposal of the Rev. W. V. Harcourt for the further prosecution of experiments on the effects of long-continued heat upon rocks, minerals, and other substances*.

4. That Dr. Turner† be requested to extend his researches into the Atomic Weights of the elementary bodies, and to report on the progress recently made in this branch of chemical science.

5. That Professor Johnston be requested to undertake a series of experiments into the comparative analysis of Iron in the different stages of its manufacture.

6. That Professor Johnston be requested to extend and revise his table of Chemical Constants, and that the sum of 20*l.* be placed at his disposal for that purpose.

7. That an examination be made into the nature and quantity of the gases given off from Thermal Waters, whether there be any variation in these respects according to season of the year, hours of the day, or condition of the atmosphere; and whether there be any changes of temperature in the same waters.

8. That the Gaseous Products which are discharged from the chimneys of smelting and other furnaces and fireplaces, be examined, at various periods of the operations carried on in them, with a view of ascertaining the compounds which are formed when the processes are most successfully conducted, and also of detecting the existence of compounds which may perhaps be new or valuable.

9. That a Committee be appointed to report their opinion as to the adoption by British chemists of an uniform system of Chemical Symbols‡.

10. That an extension of the researches commenced by Sir David Brewster into the optical properties of Minerals be recommended to the attention of chemists.

11. That Mr. Graham be requested to submit to further investigation the amount of security to be derived from the Safety-lamp, and the means of improving it.

* For a report of experiments instituted by Mr. Harcourt, in Yorkshire, at the Low Moor Iron Works, and at the Elsecar Furnace, see vol. iii.

† For a notice of the researches of Dr. Turner, see vol. ii. p. 399.

‡ For this report, see vol. iv. p. 207.

## MINERALOGY.

### *Resolutions of the Committee.*

1. That Prof. Miller be requested to determine the form and optical characters of those Crystallized Bodies which have not been previously examined, and *that chemists be invited to send him specimens of perfect artificial crystals.*

2. That Dr. Turner, Prof. Miller, Mr. Brooke, and the Rev. W. Whewell, be requested to cooperate in prosecuting and promoting the following inquiries, with a view to examine the theory of Isomorphism, and the connexion between the crystalline forms and chemical constitution of Minerals:—

I. To determine whether the angles of *varieties* of the same species (in the usual acceptation of identity of species) are identically the same, under various circumstances of colour, appearance, and locality; and if not, what are the differences?

II. To determine the chemical constitution of such varieties,—the specimens, mineralogically and chemically examined, being in all cases the same.

III. To determine what quantity of extraneous substances may be mixed with a crystalline salt, without altering its form.

IV. To determine the angles of the various species or varieties of isomorphous or plesiomorphous groups,—and their respective chemical composition.

### *Desiderata noticed in Mr. Whewell's Report.*

1. To determine the optical differences on which depend the distinctions of the different kinds of lustre, *metallic*, *adamantine*, *vitreous*, *resinous*, *pearly*.

2. To determine whether the oblique rhombic prism constitutes a real system of crystalline forms, or is a hemihedral form of the right prism.

3. To determine the limits of magnitude and simplicity in crystallometrical ratios.

4. To determine whether chemical groups are strictly *isomorphous* or only *plesiomorphous*.

5. To determine whether the angles of plesiomorphous crystals are separated by definite or by indefinite steps.

6. To determine what are the differences of chemical composition corresponding to differences of optical structure in resembling minerals, as apophyllite, tesselite, leucocyclite.

# GEOLOGY.

*Resolutions of the Committee.*

1. That it be represented to the Government that the advancement of various branches of science is greatly retarded by the want of an accurate map of the whole of the British Islands, and that the expediting the completion of the still unfinished or unpublished portions of the Ordnance Survey is much to be desired.

2. That measurements should be made, and the necessary data procured, to determine the question of the permanence or change of the relative level of Sea and Land on the coasts of Great Britain and Ireland; and that for this purpose, a sum not exceeding 100*l*. be placed at the disposal of a Sub-committee, consisting of Mr. Greenough, Mr. Lubbock, Mr. G. Rennie, Prof. Sedgwick, Mr. Stevenson, and the Rev. W. Whewell:—the measurements to be so executed, as to furnish the means of reference in future times, not only as to the relative levels of the land and sea, but also as to waste or extension of the land.

3. That Prof. Phillips be requested to draw up, with such cooperation as he may procure, a Systematic Catalogue of all the organized Fossils of Great Britain and Ireland, hitherto described, with such new species as he may have an opportunity of accurately examining*.

4. That Mr. John Taylor be requested to collect detailed sections of the Carboniferous series of Flintshire, with a view to a comparison with the same series in other parts of England;—with a view also of ascertaining the circumstances under which the Mountain Limestone is developed, after its suppression in certain coal-fields in the central parts of England.

5. That the attention of geologists be invited to those coal districts in the midland counties of England, where, the Carboniferous Limestone and Old Red Sandstone being deficient, the coal measures rest immediately on the Grauwacke and Transition rocks;—with a view to discover whether any circumstances connected with the physical structure of that part of the island can be stated, explanatory of the local absence of the two great formations above mentioned.

6. That the direction, intersection, inclination, and breadth of the non-metalliferous Fissures which cross the planes of the strata, and in some instances divide many contiguous strata,

* This catalogue is commenced, several monographs are composed. Communications, lists of organic remains, notices of localities, and *specimens of new or undescribed species*, may be addressed to Professor Phillips, Museum, York.

should be observed, in relation to the same circumstances in the dykes and mineral veins of the vicinity; with a view to ascertain whether any and what dependence there may be between these phænomena*.

7. That the quantity of Mud and Silt contained in the water of the principal rivers of Great Britain should be ascertained, distinguishing, as far as may be possible, the comparative quantity of sediment from the water at different depths, in different parts of the current, and at different distances from the mouth of the river; distinguishing also any differences in the quality of the sediment, and estimating it at different periods of the year; and that the sum of 20*l.* be placed at the disposal of the Rev. J. Yates and Mr. G. Rennie, for the purpose of these experiments.

8. That with a view to the improvement of our knowledge of the Fossil Ichthyology of the British Islands, a sum not exceeding 105*l.* be paid to Dr. Buckland, Prof. Sedgwick, and Mr. Murchison, to be applied for the purpose of assisting M. Agassiz in carrying on his Ichthyological work.

8 *a*. That a further sum of 105*l.* be applied to the same purpose.

9. That a Catalogue be formed of all known Basaltic Dykes in the United Kingdom, with an account of the direction of each in regard to the meridian, the distance of which it has been traced, and the rocks which it passes through.

10. That a list be collected of all places in which Shells of existing species have been found on dry land, with the heights of such places above the mean tide levels, and their distance from the coast, specifying the names of the several shells, and the area over which they extend.

10 *a*. That evidence be collected as to the direction and probable sources from which drifted blocks and pebbles referrible to rocks not existing in the neighbourhood, where they now occur as insulated blocks or in beds of superficial gravel, may have been derived. 10 *b*. That evidence be collected as to the form and direction of hills or ridges of superficial gravel, and the sources whence the materials of such gravel-hills may have been transported to their present place.

10 *c*. That observations be made on the direction and depth of grooves and furrows, such as are often found on the faces of hard rocks and beneath superficial deposits of drifted clay and gravel not referrible to the action of any existing currents.

* Professor Phillips has stated (see *Reports*, vol. iii. p. 654,) the results of his examination on this subject in certain parts of the North of England, and requests to be favoured with communications relating thereto.

*Desiderata noticed in Mr. Conybeare's Report.*

1. An accurate examination of the conclusions deducible from the known density of the earth, as to the solid structure and composition of the interior.

2. The attention of residents in our remote foreign dependencies to the two great questions of comparative Geology and Palæontology. 1. Is there or is there not such a general uniformity of type in the series of rock-formations in distant countries, that we must conceive them to have resulted from general causes of almost universal prevalence at the same geological æra? 2. Are the organic remains of the same geological period specifically similar in *very* remote districts, and more especially under climates actually different; or are they grouped together within narrower boundaries and under restrictions as to geographical *habitats*, analogous to those which prevail in the actual system of things?

3. An examination of the geological structure of the countries constituting the great basin of the Indus; where, if in any part of India, it is supposed a complete series of secondary strata may be expected.

4. To determine, by induction, the forces which have produced the elevation and general configuration of the land, and to investigate the dynamical laws of these forces*.

*Desideratum noticed in Mr. Taylor's Report on Mineral Veins.*

A correct account of the affinity that the contents of a vein bear to *certain* of the rocks in which the fissure may be situated.

## NATURAL HISTORY.

*Resolutions of the Committee.*

1. That a Committee be appointed for devising the means of forming and publishing a full and arranged Catalogue of works on Natural History.

2. That a Committee be appointed to obtain an exact Catalogue of the Animals and Plants inhabiting Ireland.

3. That Mr. Ball be requested to investigate the mode by

* *Reports*, vol. i. p. 408. See Mr. Hopkins's Essay on Phys. Geol. in the *Transactions of the Cambridge Philosophical Society*.

which *Echinus lividus* excavates the rocks on which it is found, and to report the results to the next Meeting.

4. That the following subjects of inquiry be recommended to the consideration of zoologists:

*a*. The use of horns in the class Mammalia; the reason of their presence in the females of some and their absence in those of other species; the connexion between their development and sexual periods; the reason of their being deciduous in some tribes and persistent in others.

*b*. The use of the lachrymal sinus in certain families of the Ruminantia*.

*c*. The conditions which regulate the geographical distribution of Mammalia.

*d*. The changes of colour of hair, feathers, and other external parts of animals; how these changes are effected in parts usually considered by anatomists as extra-vascular.

*e*. The nature and use of the secretions of certain glands immediately under the skin, above the eyes, and over the nostrils, in certain species of the *Grallatores* and *Natatores*; the nature and use of the secretion of the uropygial gland.

*f*. How long and in what manner can the impregnated ova of Fishes be preserved, for transportation, without preventing vivification when the spawn is returned to water.

*g*. Further observations on the supposed metamorphosis of Decapod *Crustacea*, with reference to the views of Thompson and Rathke †.

*h*. The situation of the sexual organs in male Spiders, and on their supposed connexion with the palpi ‡.

*i*. The use of the antennæ in Insects. Are they organs of hearing, of smell, or of a peculiar sensation?

*k*. The function of the femoral pores in Lizards, and the degree of importance due to them as offering characters for classification.

5. That botanists in all parts of Great Britain and Ireland be invited to compose and communicate to the Meetings of the Association, Catalogues of local Floras, with indications of those species which have been *recently introduced*, of those which are

* See Dr. Jacob's remarks, *Reports*, vol. iv. p. 208.

† See essays by Mr. Westwood and Mr. Thompson in *Phil. Trans.*

‡ See Mr. J. Blackwall's paper, *Reports*, vol. ii. p. 444.

*rare* or *very local*, and of those which thrive, or which have become, or are becoming extinct; with such remarks as may be useful towards determining the connexion which there may be between the *habitats* of particular plants, and the nature of the *soil and the strata upon which they grow; with statements of* the *mean winter* and *summer temperature* of the air and the water, at the highest as well as the lowest elevation at which species occur; the hygrometrical condition of the air, and any other information of an historical, œconomical, and philosophical nature.

6. That Professor Daubeny be requested to institute an extended inquiry into the exact nature of the secretions by the roots of the principal cultivated plants and weeds of agriculture; and that the attention of botanists and chemists be invited to the degree in which such secretions are poisonous to the plants that yield them, or to others; and to the most ready method of decomposing these secretions by manures or other means.

7. That Mr. Mackay be requested to institute a series of experiments to determine the limits of species in the genus *Saxifraga*, and especially of those which are natives of Ireland. Likewise, that he be requested to communicate a detail of the peculiarities of the vegetation of the east and west coasts of Scotland and of the opposite coast of Ireland.

8. That a Committee be formed to conduct a series of experiments on the growth of plants from seeds, and to preserve the results of their experiments, in order to establish the identity or confirm the specific distinctions of certain allied plants, and to communicate the results obtained from year to year at the Meetings of the Association*.

## *Desiderata noticed in Mr. Jenyns's Report on Zoology.*

1. Local Faunas, in which the structure and habits of animals, although only of a few species, shall be given with scrupulous accuracy—monographs in which species shall be investigated with a view to their exact differences, and in which the synonyms of those which have been noticed by other authors shall be distinctly made out.

2. Further attention to the Fishes and *Invertebrata* of the British coasts, and especially to the *Radiata* of Cuvier. (*Reports*, vol. iii. p. 249.)

* Mr. Don, Librarian to the Linnæan Society, has undertaken to be the channel of correspondence on this subject.

*Desiderata noticed in Professor Lindley's Report on Botany.*

1. An accurate account of the manner in which the woody part of plants is formed.

2. *An investigation of the comparative anatomy of flowerless plants, with a view to discover in them the analogy and origin of their organic structure.*

3. The cause of the various colours of plants.

4. The nature of the fæcal excretions of cultivated plants, and of common weeds; the degree in which those excretions are poisonous to the plants that yield them, or to others; the most ready means of decomposing such excretions by manures or other means.

## MEDICAL SCIENCE.

*Resolutions of the Committee.*

1. That the effects of poisons on the animal œconomy should be investigated and illustrated by graphic representations; and that a sum not exceeding 25*l.* be appropriated for this object. Dr. Roupell and Dr. Hodgkin were requested to undertake this investigation*.

2. That an experimental investigation should be made of the sensibilities of the Nerves of the Brain; and that a sum not exceeding 25*l.* should be appropriated to this object. Dr. Marshall Hall and Mr. S. D. Broughton were requested to undertake these experiments†.

3. That two Committees be appointed, one to meet in Edinburgh and the other in London, for the purpose of investigating the anatomical relations of the absorbent and nervous systems in the different classes of animals, to be illustrated by injected preparations and graphic representations, and that the sum of 25*l.* be placed at the disposal of each Committee for assisting the prosecution of these researches.

4. That two Committees be appointed, one to meet in Edinburgh and the other in Dublin, to investigate the motions and sounds of the heart, and that the sum of 50*l.* be placed at their disposal‡; and that it be an instruction to these Committees to determine whether the muscular fibres of the columnæ carneæ contract at the same precise moment as the mass of muscular fibres of the ventricle; also, what is the precise mode in which

* *Reports*, vol. iv. pp. 211 and 235. † *Reports*, vol. iii. p. 676.
‡ *Reports*, vol. iv. p. 243.

the tricuspid and mitral valves prevent the reflux of blood;—are they floated up and stretched across the auriculo-ventricular orifices, or are they drawn together to a point within the cavity of the ventricle by the action of the columnæ carneæ?

5. That two Committees be appointed, one to meet in Edinburgh, the other in London, to communicate with the London Statistical Society and the Statistical Committee of the Association relative to a registration of deaths, comprising particulars of a medical nature*.

6. That a Committee be appointed in Dublin to report on the pathology of the brain and nervous system†.

7. That, considering the importance of the inquiry instituted by Dr. Osborne on the action of cold and on the application of a particular modification of the thermometer to the measuring of refrigeration of the human body, Committees be appointed in London, Edinburgh, and Dublin to prosecute the said inquiry.

## ARTS.

*Resolutions of the Committee.*

1. That Messrs. Hodgkinson and Fairbairn be requested to undertake a series of experiments on the difference of strength and other mechanical properties of iron obtained by the hot and cold blast under similar circumstances to the nature of the coal employed, and from the same manufactory, and that a sum not exceeding 30*l.* be placed at their disposal for that purpose.

2. That the sum of 50*l.* be applied towards procuring, printing, and circulating periodical statements of the *duties* of steam-engines in Cornwall and elsewhere.

*Desideratum noticed by Professor Barlow in his Report on the Strength of Materials.*

A set of experiments on the application of a straining force on vertical columns (of timber, iron, &c.).

## STATISTICS.

*Resolutions of the Committee.*

1. That Colonel Sykes be requested to prepare for publication his valuable statistical returns, collected by himself in India,

* *Reports*, vol. iv. p. 251. † *Reports*, vol. iv.

relative to the four collectorates of the Deccan, subject to the Bombay Government.

2. That Professor Jones be requested to endeavour to obtain permission to examine the statistical records understood to exist in great number in the archives of the India House, and to prepare an account of the nature and extent of them*.

3. That Mr. Taylor be requested to draw up a series of questions upon the condition and habits of the mining population of Cornwall and Wales, with a view to obtain a complete account of the statistics of that class.

4. That Dr. Chawnor be requested to furnish to the Statistical Section of the British Association a return of the inquisitions taken before the Coroners of the county of Nottingham and elsewhere within his neighbourhood; and that it would be highly desirable to have similar returns from all the counties of England during the seven years ending 1834, and that Mr. Halswell be requested to draw up a form in which to make this record, and to obtain for the Association the returns from as large a number of districts as may be in his power.

5. That Mr. Halswell be requested to furnish returns of the Hanwell Lunatic Asylum since its commencement.

6. That inquiries into the state of education upon the plan pursued by the Statistical Society of Manchester would afford a very useful addition to statistical knowledge, and that it be recommended that such inquiries be pursued in other towns and districts, and the results arranged, for the sake of uniformity, under the same heads as those adopted by the Manchester Society†.

## DIRECTIONS FOR OBSERVING THE TIDES.

Observations of tides along the coasts of Great Britain and Ireland will be valuable, both in the construction of more accurate tide-tables, and as data toward the perfection of the theory of tides.

Observations of the tides should record particularly,—

The time in hours and minutes, and height of high-water daily, or, if convenient, every tide.

The time and height of low-water.

* Professor Jones reported to the Committee that, in pursuance of this recommendation, he had applied for access to the archives of the East India Company, and that, with their accustomed liberality, they had afforded him every facility for prosecuting his researches.

† *Reports*, vol. iv., Notices, p. 122, *infrà*.

The direction of the wind, and the height of the barometer and thermometer.

The direction and velocity of the stream of flow and ebb.

At what hour (with respect to the time of high-water and low-water) the slack-water after the stream of flood, and after the stream of ebb, respectively occur.

The height of the water must be given *from some fixed mark or line*, which should be described accurately, so that it may be easily found again at a future time. The observer ought to state the manner in which the height was measured; the manner in which the moment of high-water was fixed upon; the time employed, *whether apparent or mean solar time*, and how it was obtained.

The height of the water at the end of every minute for half an hour before the expected time of high-water, and until there can be no doubt that the time of high-water is past. Machines to dispense with this minute attention are described in the *Philosophical Transactions*, 1831, and in the *Nautical Magazine* for October, 1832*.

The uncertainty occasioned by waves may be avoided by making the observation in a chamber, to which the water has access by a small opening, or by fixing in the water an upright tube (of wood or iron, for instance), the bottom or sides of the tube being perforated; in either case an upright measuring rod, carefully graduated, and connected to a float, will rise and fall with the tide, and permit, at any moment, the height of the water to be read off against the collar through which it works. This rod may be so constructed as to leave a moveable index at the highest and lowest points.

A long series of continued observations can alone be of use towards the determination of the dependence of the time, height, and other circumstances of high- and low-water upon the places and distances of the sun and moon; but a smaller number of observations will often be sufficient to determine the *establishment* of any place, with more or less accuracy, according to the number of observations; and the best mode of doing this is by comparative observations with some place of which the establishment is accurately known, or where observations are continually carried on. A few sets of comparative observations of neighbouring places will give the *relative* time of high-water at these places with considerable accuracy; and thus the motion of the tide-wave and the arrangement of the *cotidal lines*, (or lines

* Tide-gauges may be seen in operation at St. Katharine's Docks, London. An excellent one has lately been set up near Bristol by the Literary and Philosophical Institution of that city.

along which it is high-water at the same instant,) will be discovered. It would be very desirable for those who have the opportunity, to combine, so as to effect the detailed description of the tides through some small extent of coast, such as that which has been effected by M. Daussy for the west coast of France.

## DIRECTIONS FOR OBSERVING THE AURORA BOREALIS.

Notwithstanding the attention which has been paid to the phænomena of the Aurora Borealis, and the various hypotheses which have been imagined to explain them, it will be found that there is a want of information on the points which are most necessary as bases of induction; and the British Association have therefore been induced to appoint a Committee in the express view of directing observers to the really important features of this meteor, and of obtaining, by a system of contemporaneous observation, data which experience shows cannot be derived from insulated exertion.

The following are the most important points which demand the attention of observers:

1. The elevation of the auroral arches and streamers above the surface of the earth.
2. The determination of the question whether the auroral exhibition is accompanied by sound.
3. The existence of recurring periods of frequency and brilliancy in the Aurora.
4. The influence of arches, streamers, and other auroral phænomena upon the magnetic needle.

1. It is recommended to all who intend to observe auroras to make themselves well acquainted with the names of all the principal stars to the north of the equator, especially those which do not set here. This will be most easily done by studying a celestial globe. Good maps of the stars may also be consulted with advantage. Either the proper names or the Greek characters with the name of the constellation will be sufficient.

Persons who may prefer to determine the angular elevation and position of the arches and streamers by graduated instruments, must be supposed well accustomed to the use of them; they may, however, be reminded that telescopic sights are for this purpose useless, and that steady instruments, which can be handled with ease and expedition, are much more available for

observations of these faint and often fluctuating meteors, than others of a more refined construction.

2. It is recommended that a magnetic needle be kept in a proper place, suspended by a silk fibre or slender hair (a point-support not being delicate enough), and so mounted, that deviations can be observed to the accuracy of 1′. It has been found convenient to fix in a garden a stone pedestal, on which, at three invariable points, the frame of the magnetic needle rests under a glass cover. The needle, 9 inches long, and of such a weight as to perform about 10 vibrations in a minute, is suspended by one slender hair. There are simple contrivances to steady the needle when required, and to adjust the length of the suspending hair. The scale is divided in degrees for 30° on each side of the centre, and in 10′ for 1° on each side. There is no vernier, but the place of the needle on the scale is read off with great ease by looking through a fixed magnifying-glass from an opening at some height above, so as to avoid sensible parallax. Professor Christie has described more complete apparatus for this purpose in the *Journal of the Royal Institution*, New Series, vol. ii. p. 278. The observer must leave his watch with the assistant, very carefully remove all keys, knives, and other things containing iron from his dress, and all loose iron tools and utensils to at least 20 feet distance from the needle. If these precautions are not scrupulously attended to, the results will be fallacious. It is proper to caution the observer that there is a *regular daily* variation of the needle, independent of the Aurora.

*Dipping-needles*, unless constructed with the utmost care, cannot be considered very satisfactory instruments; yet, if their suspension be sufficiently delicate, they may probably very well answer for observations during Aurora, of which the object is to determine, not the *absolute dip of the needle*, but the *change of dip* occasioned by the Aurora. The same precautions of one certain position, removal of iron, &c. are necessary, as in the use of the horizontal needle.

3. It is recommended that arrangements be made for ascertaining the error of a watch. If near an observatory of any kind, the watch should be compared with the transit clock there immediately after an Aurora; if there is a good meridian line, or good dial, the error of the watch on mean time should be found as soon as possible; if a watchmaker in the neighbourhood has a good regulator, the watch should be adjusted by it, and the mode of keeping the regulator should be ascertained; if a mail-coach from London passes near, the guard's watch may be consulted. The longitude of the place of observation

should be ascertained from a map or otherwise. The attention of observers is especially called to the point of ascertaining the time correctly, as it is one of the most important points, and the one which probably will require the longest forethought.

4. In default of intelligence of an Aurora, the observer should go out of doors to some station where the horizon is pretty clear, and look about every evening at 10, Greenwich mean solar time, as near as may be. He should keep a journal, noting for this time every evening whether there was an Aurora; a single word will be sufficient.

5. As soon as the observer perceives or receives notice of an Aurora, he should, if accustomed to magnetic observations, observe the magnetic needle, and should go to some commanding situation with his watch in his hand, and a note-book. A person so prepared will have little difficulty in fixing on the appearances most worthy of notice. We may, however, point out the following:

I. If there is an arch, the positions of its two boundaries, its terminations, &c. should be noted by the way in which they pass among the stars (the proportion of distances between the stars admitting of very accurate estimation by the eye). If, as rarely happens, the sky is cloudy, the observer may notice the elevation and extent of the arch by moving till it appears to touch the top of some terrestrial object, noting his situation as well as he can, and the next day observing with a theodolite the angular elevation and azimuth of the object; or ascertaining the height and horizontal distance, and thence computing the angular elevation, and observing the azimuth by a common compass; but it is recommended not to adopt this method when the observation of stars is practicable. Notice should be taken whether one edge is better defined than the other; whether there is clear sky or dark cloud above or below; whether it terminates at the end in sky or in cloud; whether there is any dark band in it; whether in its general composition it is uniform or striated; whether stars can be seen through it, &c.

II. If any change takes place in the situation or appearance of the arch, the observer should instantly look at his watch and set down the time, and then proceed to note the change.

III. If there are beams or streamers, the time should be noted; then their position among the stars; then their height among the stars; their motion (whether verti-

tical or horizontal); the velocity of motion (by the time of passing from one star to another); their changes; their permanency; whether they appear to affect the arch, or to be entirely in front of it.

IV. If there are any black clouds in the luminous region, notice should be taken whether the streamers seem to have any relation to them; whether the arch seems to have any relation to them; whether and in what manner they increase or disappear.

V. If there are waves or flashes of light, the observer should notice the time of beginning and of finishing; the general extent of the flashes (up and down, as well as right and left); whether the flash is a real progress of light or successive illumination of different places; and anything else that strikes him.

VI. The existence and change of colours will, of course, be noticed.

VII. From time to time the needle should be observed. If there are two persons capable of accurate observation, it is most desirable that one should steadily watch the needle and the other the sky.

6. When all is over, the observer should immediately put his rough notes in form, and as soon as possible should compare his watch with the regulator, or other authority for his time.

7. The next day he should, from a celestial globe, take the altitudes and azimuths by means of the stars; he should reduce his observed time to Greenwich mean solar time, and he should append these reductions to his rough observations. In this state the observations are fit for publication, and adapted for immediate use. It is desirable that they should be transmitted without delay to the *Assistant Secretary of the British Association, Museum, York.*

## FALLING STARS.

M. Quetelet's mode of observing these meteors is contained in the following extract of a letter from him:—

I take my station out of doors, in a situation which commands a good view of the sky, with a good map of the heavens spread out before me. When a falling star appears, I mark on the map the point of its commencement, the line of its course amongst the nearest stars, and the point where it vanished. This is done by an *arrow line*, which marks the apparent direction and extent of the course of the meteor; the time is carefully noted; a num-

ber of reference is placed on the line, and the principal circumstances of the meteor are then registered in tables of the following form :—

| Epoch. | No. | Magnitude relative to stars. | Duration of the appearance. | Time of * appearance. | Remarks. |
|---|---|---|---|---|---|
| Aug. 29. | 1 | 2 | 2″.5 | $10^h\ 6'\ 4''$. | |

It is important to remark, whether the falling star leaves, or not, any *trace* of its course, as sometimes happens, in the form of reddish scintillations; the condition of the atmosphere, as determined by the usual instruments, should be noted; the time must be accurately ascertained; more than one observer should be engaged at each station, because the meteors sometimes succeed one another very quickly, and the duration of the phænomenon is too short to permit one person to note the position, time, and circumstances of each, with sufficient precision*.

## CONSTANTS OF NATURE AND ART.

"Amongst those works of science which are too large and too laborious for individual efforts, and are therefore fit objects to be undertaken by united Academies, I wish to point out one which seems eminently necessary at the present time, and which would be of the greatest advantage to all classes of the scientific world.

" I would propose that its title should be *The Constants of Nature and of Art*. It ought to contain all those facts which can be expressed by numbers in the various sciences and arts." (*Babbage, Edinburgh Journal of Science, N.S., No.* 12.)

The following extracts from Mr. Babbage's general plan of contents will exemplify the objects and arrangement of the proposed work.

These contents should consist of—

1. All the constant quantities belonging to our system;—as distance of each planet,—period of revolution,—inclination of orbit, &c.—proportion of light received from the sun,—force of gravity on the surface of each, &c.

2. The atomic weight of bodies.

3. List of the metals, with columns for specific gravity,—electricity,—tenacity,—specific heat,—conducting power for

* Contemporaneous observations are especially desirable on this subject; persons desirous of undertaking this investigation are therefore requested to apply to a member of the Auroral Committee, or to the Assistant Secretary at York, for information of the evenings and hours appointed for this purpose.

heat,—conducting power for electricity,—melting-point,—refractive power,—proportion of rays reflected out of 1000,—at an incidence of 90°.

4. Specific gravities of all bodies.

5. List of Mammalia, with columns for height,—length,—weight,—weight of skeleton,—weight of each bone,—its greatest length,—its smallest circumference,—its specific gravity,—number of young at a birth,—number of pulsations per minute,—number of inspirations per minute,—period of blindness after birth,—of sucking,—of maturity,—temperature,—average duration of life,—proportion of males to females produced, &c. &c.

## SYNOPSIS OF SUMS APPROPRIATED TO SCIENTIFIC OBJECTS.

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# ADDRESS

BY

# PROFESSOR SIR WILLIAM R. HAMILTON.

It has fallen to my lot, Gentlemen, as one of your Secretaries for the year, to address you on the present occasion. The duty would, indeed, have been much better discharged had it been undertaken by my brother secretary; but so many other duties of our secretaryship had been performed almost entirely by him, that I could not refuse to attempt the execution of this particular office, though conscious of its difficulty and its importance. For if we may regard it as a thing established now by precedent and custom that an annual address should be delivered, it is not, therefore, yet, and I trust that it will never be, an office of mere cold routine, a filling up of a vacant hour, on the ground that the hour must be some way or other got rid of. You have not left your homes—you have not adjourned from your several and special businesses—you have not gathered here, to have your time thus frittered away in an idle and unmeaning ceremonial. There ought to be, and there is, a reason that some such thing should be done; that from year to year, at every successive reassembling, an officer of your body should lay before you such an address; and in remembering what this reason is, we shall be reminded also of the spirit in which the duty should be performed. The reason is the fitness and almost the necessity of providing, so far as an address can provide, for the permanence and progression of the body, by informing the new members, and reminding the old, of the objects and nature of the Association, or by giving utterance to at least a few of those reflections which at such a season present themselves respecting its progress and its prospects; and it is a valid reason, and deserves to be acted upon now, however little may have been left unsaid in the addresses of my predecessors in this office. For if even amongst the members who have attended former meetings, and have heard those eloquent addresses delivered by former secretaries, it is possible that

some may have been so dazzled by the splendour of the spectacle, and so rapt away by the enthusiasm of the time, as to have given but little thought to the purport and the use, the meaning and the function of the whole; much more may it be presumed that of the several hundred persons who have lately joined themselves as new members to this mighty body, there are some, and even many, who have reflected little as yet upon its characteristic and essential properties, and who have but little knowledge of what it has been, and what it is, and what it may be expected to become. First, then, the object of the Association is contained in its title; it is the advancement of science. Our object is not literature, though we have many literary associates, and though we hail and love as brethren those who are engaged in expressly literary pursuits, and who are either themselves the living ornaments of our land's language, or else make known to us the literary treasures of other languages, and lands, and times. Our object is not religion in any special sense, though respect for religious things, and religious men, has always marked these meetings, and though we are all bound together by that great tie of brotherhood which unites the whole human family as children of one Father who is in heaven. Still less is our object politics, though we are not mere citizens of the world, but are essentially a British Association of fellow-subjects and of fellow-countrymen, who give, however, glad and cordial welcome to those our visitors who come to us from foreign countries, and thankfully accept their aid to accomplish our common purpose. That common purpose, that object for which Englishmen, and Scotchmen, and Irishmen have banded themselves together in this colossal Association, to which the eyes of the whole world have not disdained to turn, and to see which, and to raise it higher still, illustrious men from foreign lands have come, IS SCIENCE; the acceleration of scientific discoveries, and the diffusion of scientific influences. And if it be inquired how is this aim to be accomplished, and through what means, and by what instruments and process we as a body hope to forward science—the answer briefly is, that this great thing is to be done by us through the agency of the social spirit, and through the means, and instruments, and process which are contained in the operation of that spirit. We meet, we speak, we feel *together now*, that we may *afterwards* the better think and act and feel *alone*. The excitement with which this air is filled will not pass at once away; the influences that are now among us will not (we trust) be transient, but abiding; those influences will be with us long—let us hope that they will never leave us; they will cheer, they will animate us still, when this brilliant week is over; they will go with us to our separate abodes, will attend us on our separate

journeys; and whether the mathematician's study, or the astronomer's observatory, or the chemist's laboratory, or some rich distant meadow unexplored as yet by botanist, or some untrodden mountain-top, or any of the other haunts and homes and oracular places of science, be our allotted place of labour till we meet together again, I am persuaded that those influences will operate upon us all, that we shall all remember this our present meeting, and look forward with joyful expectation to our next reassembling, and by the recollection, and by the hope, be stimulated and supported. It is true, that it is the individual man who thinks and who discovers; not any aggregate or mass of men. Each mathematician for himself, and not any one for any other, not even all for one, must tread that more than royal road which leads to the palace and sanctuary of mathematical truth. Each, for himself, in his own personal being, must awaken and call forth to mental view the original intuitions of time and space; must meditate himself on those eternal forms, and follow for himself that linked chain of thought which leads, from principles inherent in the child and the peasant, from the simplest notions and marks of temporal and local site, from the questions when and where, to results so varied, so remote, and seemingly so inaccessible, that the mathematical intellect of full-grown and fully cultivated man cannot reach and pass them without wonder, and something of awe. Astronomers, again, if they would be more than mere artizans, must be more or less mathematicians, and must separately study the mathematical grounds of their science; and although in this as in every other physical science, in every science which rests partly on the observation of nature, and not solely on the mind of man, a faith in testimony is required, that the human race may not be stationary, and that the accumulated treasures of one man or of one generation of men may not be lost to another; yet even here, too, the individual must act, and must stamp on his own mental possessions the impress of his own individuality. The humblest student of astronomy, or of any other physical science, if he is to profit at all by his study, must in some degree go over for himself, in his own mind, if not in part with the aid of his own observation and experiment, that process of induction which leads from familiar facts to obvious laws, then to the observation of facts more remote, and to the discovery of laws of higher orders. And if even this *study* be a personal act, much more must that *discovery* have been individual. Individual energy, individual patience, individual genius, have all been needed, to tear fold after fold away, which hung before the shrine of nature; to penetrate, gloom after gloom, into those Delphic depths, and force the reluctant Sibyl to utter her oracular responses. Or if we look from nature up to nature's

God, we may remember that it is written—"Great are the works of the Lord, sought out of all those who have pleasure therein." But recognising in the fullest manner the necessity for private exertion, and the ultimate connexion of every human act and human thought with the personal being of man, we must never forget that the social feelings make up a large and powerful part of that complex and multiform being. The affections act upon the intellect, the heart upon the head. In the very silence and solitude of its meditations, still genius is essentially sympathetic; is sensitive to influences from without, and fain would spread itself abroad, and embrace the whole circle of humanity, with the strength of a world-grasping love. For fame, it has been truly said, is love disguised. The desire of fame is a form of the yearning after love; and the admiration which rewards that desire, is a glorified form of that familiar and every-day love which joins us in common life to the friends whom we esteem. And if we can imagine a desire of excellence for its own sake, and can so raise ourselves *above* (Well if we do not in the effort sink ourselves *below*) the common level of humanity, as to account the aspiration after fame only "the last infirmity of noble minds," it will still be true that in the greatest number of cases, and of the highest quality,

> Fame is the spur that the clear spirit doth raise,
> To scorn delights, and live laborious days.

That mysterious joy—incomprehensible if man were wholly mortal—which accompanies the hope of influencing unborn generations; that rapture, solemn and sublime, with which a human mind, possessing or possessed by some great truth, sees in prophetic vision that truth acknowledged by mankind, and itself long ages afterward remembered and associated therewith, as its interpreter and minister, and sharing in the offering duly paid of honour and of love, till it becomes a power upon the earth, and fills the world with felt or hidden influence; that joy which thrills most deeply the minds the most contemptuous of mere ephemeral reputation, and men who care the least for common marks of popular applause or outward dignity—does it not show, by the revival, in another form, of an instinct seemingly extinguished, how deeply man desires, in intellectual things themselves, the sympathy of man? If then the *ascetics* of science—if those who seem to shut themselves up in their own separate cells, and to disdain or to deny themselves the ordinary commerce of humanity—are found, after all, to be thus influenced by the *social spirit*, we can have little hesitation in pronouncing that to the operation of this spirit must largely be ascribed *the labours of ordinary minds*; of those who do not even affect or seem

to shun the commerce of their kind; who accept gladly, and with acknowledgedjoy, all present and outward marks of admiration or of sympathy, and who are willing, and confess themselves to be so, to do much for immediate reward, or speedy though perishing reputation. Look where we will, from the highest and most solitary sage who ever desired "the propagation of his own memory," and committed his lonely labours to the world, in full assurance that an age would come, when that memory would not willingly be let to die, down to the humblest labourer who was ever content to cooperate outwardly and subordinately with others, and hoped for nothing more than present and visible recompense, we still perceive the operation of that social spirit, that deep instinctive yearning after sympathy, to use the power, and (if it may be done) to guide the influences of which, this British Association was framed. Thus much I thought that I might properly premise, on the social spirit in general, and its influence upon the intellect of man; since that is the very bond, the great and ultimate reason, of this and of all other similar associations and companies of studious men. But you may well expect that in the short remaining time which your leisure this evening can spare, I should speak more especially, and more definitely, of this British Association in particular. And here it may be right to adopt in part a more technical style, and to enter more minutely into detail, than I could yet persuade myself to do, till I had eased myself in some degree of those overflowing emotions, which on such an occasion as this could hardly be altogether suppressed. Presuming, therefore, that some one now demands, how this Association differs from its fellows, and what peculiar means it has of awakening and directing to scientific purposes the power of the social spirit; or why, when there were so many old and new societies for science, it was thought necessary or expedient to call this society also into being: I proceed to speak of some of the characteristic and essential circumstances of this British Association, which contain the answer to that reasonable demand. First, then, it differs in its magnitude and universality from all lesser and more local societies. So evidently true is this, that you might justly blame me if I were to occupy your time by attempting any formal proof of it. What other societies do upon a small scale, this does upon a large; what others do for London, or Edinburgh, or Dublin, this does for the whole triple realm of England, Scotland, and Ireland. Its gigantic arms stretch even to America and India, insomuch that it is commensurate with the magnitude and the majesty of the British empire, on which the sun never sets; and that we hail with pleasure, but without surprise, the enrolment of him among our members who represents the sovereign

here, and is to us the visible image of the head of that vast empire; and the joy with which we welcome to our assemblies and to our hospitality those eminent strangers who have come to us from foreign lands, rises almost above the sphere of private friendship, and partakes of the dignity of a compact between all the nations of the earth. Forgive me that I have not yet been able to speak calmly in such a presence, and on such a theme. But it is not merely in its magnitude and universality, and consequently higher power of stimulating intellect through sympathy, that this Association differs from others. It differs also from them in its constitution and details; in the migratory character of its meetings, which visit, for a week each year, place after place in succession, so as to indulge and stimulate all, without wearying or burdening any; in encouraging oral discussion, throughout its several separate sections, as the principal medium of making known among members the opinions, views, and discoveries of each other; in calling upon eminent men to prepare reports upon the existing state of knowledge in the principal departments of science; and in publishing only abstracts or notices of all those other contributions which it has not as a body called for; in short, in attempting to induce men of science to work more together than they do elsewhere, to establish a system of more strict cooperation between the labourers in one common field, and thus to effect, more fully than other societies can do, the combination of intellectual exertions. In other societies, the constitution and practice are such, that the labours of the several members are comparatively unconnected, and few attempts are systematically made to combine and harmonize them together; so that if we except that general and useful action of the social spirit upon the intellect of which I have already spoken, and the occasional incitement to specific research, by the previous proposal of prizes, there remains little beyond the publication of Transactions, whereby they seek as bodies to cooperate in the work of science. In them an author, of his own accord, hands in a paper; the title and subject are announced; it is referred to a Committee for examination, and if it be approved of, it is published at the expense of the society. This is a very great and real good, because the most valuable papers are seldom the most attractive to common purchasers, and because the authors of these papers are rarely able to defray from their own funds the cost of an expensive publication. There is no doubt that if it had not been for this resource, many essays of the greatest value must have been altogether suppressed, for want of pecuniary means. Besides, the approbation of a body of scientific men, which is at least partially implied in their undertaking to publish a paper, however limited and guarded it may be by their disclaimer of

corporate responsibility, cannot fail to be accounted a high and honourable reward; and one, of which the hope must much assist to cheer and support the author in his toils, by virtue of the principle of sympathy. It is known, and (I believe) was mentioned in an address to this Association, at one of the former meetings, that the Principia and Optics of Newton were published at the request of the Royal Society of London. Newton, indeed, might well have thought that those works did not need that sanction, if the meekness of his high faculties had permitted him to judge of himself as all other men have judged of him; but our gratitude is not therefore due the less to the Society whose request prevailed over his own modest reluctance, and procured those treasures for that and for every age. It must be added that the Royal and Astronomical Societies print abstracts of their communications, for speedy circulation among their members, which is a useful addition to the service done in publishing the papers themselves, and is an example well worthy of being followed by all similar institutions; and that the Royal Society has even gone so far as to procure and print, in at least one recent instance (I mean in the case of a paper of Mr. Lubbock's), and perhaps also in some other instances, a report from some of its members, on a memoir presented by another, thus imitating an excellent practice of the Institute of France, which has probably contributed much to the high state of science in that country. This last procedure, and doubtless other acts of some other scientific societies, such as the discussions in the Geological Society, the lending of instruments by the Astronomical Society to its members, and the occasional exhibition of models and experiments by members to the body, in the Irish and other institutions, are examples of direct cooperation; and perhaps there is nothing to prevent such cases being greatly multiplied hereafter. But admitting freely these and other claims of the several societies and academies of the empire to our gratitude for their services to science, and accounting it a very valuable privilege to belong, as most of us do, to one or other of those bodies, and acknowledging that there is much work to be done which can only be done by them, we must still turn to this British Association, as the body which is *cooperative* by eminence.—The *discussions* in its sections are more animated, comprehensive and instructive, and make minds which were strangers, more intimately acquainted with each other, than can be supposed to be the case in any less general body; the *general meetings* bring together the cultivators of all different departments of science; and even the less formal *conversations*, which take place in its halls of assembly during every pause of business, are themselves the working together of mind with mind, and not only excite but *are* co-

operation. Express requests also are systematically made to individuals and bodies of men, to cooperate in the execution of particular tasks in science, and these requests have often been complied with. But more perhaps than all the rest, the reports which it has called forth on the existing state of the several branches of knowledge are astonishing examples of industry and zeal exerted in the spirit and for the purpose of cooperation. No other society, I believe, has yet ventured to call on any of its members for any such report, and indeed it would be a difficult, perhaps an invidious thing, for any one of the other societies or academies so to do. For such a report should contain a large and comprehensive view of the treasures of all the academics; and would it not be difficult for a zealous member of any one of them, undertaking the task at the request of his own body, to form and to express that view with all the impartiality requisite? Would there not be some danger of a bias, in some things to palliate the defects of his own particular society, and in other things to exalt beyond what was strictly just, its true and genuine merits? But a body like the British Association which receives indeed all communications, but publishes (except by abstract) none, save only those very reports which it had previously and specially called for,—a body such as this, and governed by such regulations, may hope, that standing in one common relation to all the existing academies, and not belonging to the same great class of societies publishing papers, the members whom it has selected for the task may come before it to report what has resulted from the labours of all those different societies, without any excessive depression or any undue exultation, and in a more unbiassed mood of mind than would be possible under other circumstances. Accordingly the reports already presented by those eminent men who were selected for the office, (and rightly so selected, because a comprehensive mind was not less needed than industry,) appear to have been drawn up with as much impartiality as diligence; they comprise a very extensive and perfect view of the existing state of science in most of its great departments: and if in any case they do not quite bring down the history of science to this day (as certainly they go near to do), they furnish some of the best and most authentic materials to the future writer of such history. But we should not only underrate the value of those reports, but even quite mistake the character of that value if we were to refer it all to its connexion with distant researches, and some unborn generation. They will, indeed, assist the future historian of science; but it was not solely, nor even chiefly for that purpose they were designed, nor is it solely or chiefly for that purpose which they will answer. They belong to our own age; they are the property of ourselves as well as of our

children. To stimulate the living, not less than to leave a record to the unborn, was hoped for, and will be attained, through those novel and important productions. In holding up to us a view of the existing state of science, and of all that has been done already, they show us that much is still to be done, and they rouse our zeal to do it. Can any person look unmoved on the tablet which they present of the brilliant discoveries of this century, in any one of the regions of science? Can he see how much has been achieved, what large and orderly structures have been in part already built up, and are still in process of building, without feeling himself excited to give his own aid also in the work, and to be enrolled among the architects, or at least among the workmen? or can any person have his attention guided to the many wants that remain; can he look on the gaps which are still unfilled, even in the most rich and costly of those edifices (like the unfinished window that we read of in the palace of eastern story), without longing to see those wants supplied, that palace raised to a still more complete perfection; without burning to draw forth all his own old treasures of thought, and to elaborate them all into one new and precious offering? —The volume containing the reports which were presented at the last meeting of the Association has been published so very recently, that it is perhaps scarcely yet in the hands of more than a few of the members; some notice of its contents may therefore be expected from me now, though the notice which I can give must of necessity be brief and inadequate. I shall speak first of two reports, which may in a certain sense be said to be on foreign science. Science, indeed, as has been well remarked, is not properly of any country; but men of science are, and in studying the works of their brethren of foreign nations, they at once increase their own stock of knowledge, and cultivate those kindly feelings of general good will, which are among the very best results of all our studies, and of all our assemblings together. The first report of the volume is that which Professor Rogers, of Philadelphia, has presented, at the request of the British Association, upon American Geology. The kindness of an eminent British Geologist, whose name would command attention if I thought myself at liberty to mention it, and whom I had requested to state to me in writing his opinion on this report, enables me to furnish you with a notice respecting its nature, which I shall accordingly read, instead of presuming to substitute any remarks of my own on the subject.

"The object proposed by Professor Rogers was to convey a clear summary of what had been ascertained concerning the geology of America, whether the knowledge acquired had been communicated to the public or not. This is not very different from the object contem-

plated by other reporters; but in the execution of the report it is found that a marked peculiarity arises. For the far greater portion of the report contains the result of Mr. Rogers's own reasonings on data, many of which appear for the first time in his essay. It has therefore more the character of a memoir than of an ordinary report. Were any one to adopt this plan in treating of the state of European geology he might be blamed, because the value of such a report would consist in the discussion of a vast mass of published data, and in the comparison of theoretical notions proposed by persons of high reputation. But in treating of America this was not the case; because, first, little authentic was known in Europe on the subject—second, there are few American authors of high repute in geology. This character of originality is certainly well supported by the author's own researches, and it is not surprising if his work contains some errors, still less remarkable that it should have excited some opposition at home. But the writer of the report has really taken much pains, has exhibited much patience, and has brought to his task a competent knowledge of European geology. It has certainly cleared our notions of the general features of American geology, and particularly augmented our positive knowledge of the more recent deposits, as regards organic remains, mineral characters, and geographical features. It is to be continued."

The other report which I alluded to, as almost entitled to be called a report on foreign science, is the report of the Rev. Mr. Challis on the theory of capillary attraction, which is a sequel to that presented at Cambridge on the common theory of fluids, and which the author proposes to follow up hereafter by another report on the propagation of motion as affected by the development of heat. Mr. Challis remarks, that while many questions in physics are to be resolved by unfolding through deductive reasoning the consequences of facts actually observed, there is also another class of questions in physical science, in which the facts that are to be reasoned from are not phænomena; for example, the fact of universal gravitation for which the evidence is inductive indeed, but yet essentially mathematical, the fact not coming itself under the cognisance of any of our senses, although its mathematical consequences are abundantly attested by observations. Mr. Challis goes on to say—"The great problem of universal gravitation, which is the only one of this class that can be looked upon as satisfactorily solved, relates to the large masses of the universe, to the dependence of their forms on their own gravitation, and the motions resulting from their actions on one another. The progress of science seems to tend towards the solution of another of a more comprehensive nature, regarding the elementary constitution of bodies and the forces by

which their constituent elements are arranged and held together. Various departments of science appear to be connected together by the relation they have to this problem. The theories of light, heat, electricity, chemistry, mineralogy, crystallography, all bear upon it. A review, therefore, of the solutions that have been proposed of all such questions as cannot be handled without some hypotheses respecting the physical condition of the constituent elements of bodies, would probably conduce by a comparison of the hypotheses towards reaching that generalization to which the known connexion of the sciences seems to point." The author finally remarks, that "questions of this kind have of late largely engaged the attention of some French mathematicians, and the nature of their theories, and the results of the calculations founded on them, deserve to be brought as much as possible into notice." Acting upon these just views, Mr. Challis has accordingly performed, for the British Association and for the British public, the important office of reviewing and reporting upon those researches of Laplace, Poisson, and Gauss, respecting the connexion of molecular attraction, and of the repulsion of heat, with the ascent of fluids in tubes, which give to his report so much of that foreign character which I have already ventured to ascribe to it; yet, it is just to add, and, indeed, Mr. Challis does so, that as Newton first resolved the mathematical problem of gravitation, in its bearings on the motion of a planet about the sun, and went far to resolve the same extensive problem in its details of perturbation also; he likewise first resolved a problem of molecular forces, and clearly foresaw and foretold the extensive and almost universal application of such forces to the mathematical expianation of the most varied classes of phænomena; and that the theory of capillary attraction, in particular, has received some very valuable illustrations in England from the late Dr. Thomas Young. I ought to mention that a very interesting report, on the foreign mathematical theories of electricity and magnetism was read in part this morning to the mathematical and physical section, by the Rev. Mr. Whewell.

The next report after that of Mr. Challis in the volume, is the report I have already alluded to, by Professor Lloyd, on the progress and present state of physical optics; respecting which I should have much to say, if I did not fear to offend the modesty of the author, and were not restrained by the recollection that he is a member of the same University with myself, and a countryman and friend of my own. I shall therefore simply express my belief, that no person who shall hereafter set about to form an opinion of his own on the question between the two theories of light, will think himself at liberty to dispense with the

study of this report. I may add that it also, as well as that of Mr. Challis, draws largely from foreign stores; but if Huygens was the first inventor, and Fresnel the finest unfolder, and Cauchy the profoundest mathematical dynamician, of the theory of the propagation of light by waves; and if the names of Malus, and Biot, and Arago, and Mitscherlich, and other eminent foreigners are familiar words in the annals of physical optics, we also can refer, among our own illustrious dead, to names enshrined in the history of this science—to the names of Newton, and Wollaston, and Young—and among our living fellow-countrymen and fellow-members of this Association, (unhappily not present here,) we have Brewster and Airy to glory of. It should be mentioned that the author of the report has himself made contributions to the science of light, more valuable than any one could collect from the statements in the report itself, and that important communications in that science are expected to be made during the present week, by Professor Powell, to a general meeting, and by Mr. MacCullagh to the physical section.

(The Secretary here read a notice, which he had procured from a scientific friend, of the report by Professor Jenyns on zoology; and afterwards continued his own remarks, as follows:)

The remaining reports in the new volume are those by Mr. Rennie on hydraulics; by Dr. Henry of Manchester, on the laws of contagion; and by Professor Clark of Cambridge, on animal physiology, and especially on our knowledge respecting the blood. Mr. Rennie's report contains, I believe, new facts from the manuscripts of his father, and is in other ways a valuable statement, industriously drawn up, of the recent improvements in the practice of hydraulics, to the theory of which science it is to be lamented that so little has lately been added: and without pretending to judge myself of the merits of the two other reports, I may mention them as compositions which I know to have interested persons, with whose professional and habitual pursuits they have no close connexion, and therefore, as an instance of the accomplishment of one great end proposed by our Association, that of drawing together different minds, and exciting intellectual sympathy. The other contents of the volume are accounts of researches undertaken at the request of the Association, notices in answers to queries and recommendations of the same body, and miscellaneous communications. Of these, it is of course impossible to speak now; your time would not permit it. Yet, perhaps, I ought not to pass over the mention of one particular recommendation which has happened to become the subject of remarks elsewhere—I mean that recommendation which advised an application to the Lords of the Treasury for a grant of money, to

be used in the reduction of certain Greenwich observations, the result of which recommendation is noticed in the volume before us. In all that I have hitherto said respecting this Association, I have spoken almost solely of its internal effects, or those which it produces on the minds and acts of its own members. But it is manifest that such a society cannot fail to have also effects which are external, and that its influence must extend even beyond its own wide circle of members. It not only helps to diffuse through the community at large a respect and interest for the pursuits of scientific men, but ventures even to approach the throne, and to lay before the King the expression of the wishes of this his Parliament of science, on whatever subject of national importance belongs to science only, and is unconnected with the predominance in the state of any one political party. It was judged that the reduction of the astronomical observations on the sun and moon, and planets, which had been accumulating under the care of Bradley and his successors, at the Royal and national Observatory of Greenwich, since the middle of the last century, but which, except so far as foreign astronomers might use them, had lain idle and useless till now, to the great obstruction of the advance of practical as well as theoretical science, was a subject of that national importance, and worthy of such an approach to the highest functionaries of the state. It happened that I was not present when the propriety of making this application was discussed, so that I do not know whether the authority of Bessel was quoted. That authority has not at least been mentioned, to my knowledge, in any printed remarks upon the question, but as it bears directly and powerfully thereupon, you will permit me, perhaps, to occupy a few moments by citing it.

Professor Bessel of Koenigsberg, who, for consummate union of theory and practice, must be placed in the very foremost rank, may be placed perhaps at the head of astronomers now living and now working, published not long ago that classical and useful volume, the *Tabulæ Regiomontanæ*, which I now hold in my hand. In the introduction to this volume of tables, Bessel remarks, that "the present knowledge of the solar system has not made all the progress which might have been expected from the great number and goodness of the observations made on the sun, and moon, and planets, from the times of Bradley down. It may, indeed, be said with truth, that astronomical tables do not err now by so much as whole minutes from the heavens; but if those tables differ by more than five seconds now, after using all the present means of accurate reduction, from a well-observed opposition of a planet (for example), their error is as manifest and certain now as an error exceeding a minute was, in a former state of astronomy—and the

discrepancies between the present tables and observations are not uncommonly outside that limit. The cause is doubtful. Errors of observation to such amount they cannot be; and therefore they can only arise from some wrong method of reduction, or wrongly assumed elliptic elements or masses of the planets, or insufficiently developed formulæ of perturbation, or else they point to some disturbing cause, which still remains obscure, and has not yet been reached by the light of theory. But it ought surely to be deemed the *highest problem of astronomy*, to examine with the utmost diligence into that which has been often said, but not as yet in every case sufficiently established, whether theory and experience do really always agree. When the solution of this weighty problem shall have been most studiously made trial of, in all its parts, then either will the theory of Newton be perfectly and absolutely confirmed, or else it will be known beyond all doubt that in certain cases it does not suffice without some little change, or that besides the known disturbing bodies there exist some causes of disturbance still obscure." And then after some technical remarks, less connected with our present subject, Bessel goes on to say, "To me, considering all these things together, it appears to be of the *highest moment* (*plurimum valere*) towards our future progress in the knowledge of the solar system, to reduce into catalogues as diligently as can be done, according to one common system of elements, *the places of all the planets observed since* 1750, than which labour, I believe that no other now will be of greater use to astronomy" (..*quo labore nullum credo nunc majorem utilitatem Astronomiæ allaturum esse*). Such is the opinion of Bessel; but such is not the opinion of an anonymous censor, who has written of us in a certain popular review. To *him* it seems a matter of little moment that old observations should be reduced. Nothing good, *he* imagines, can come from the study of those obsolete records. It may be very well that thousands of pounds should continue to be spent by the nation, year after year, in keeping up the observatory at Greenwich; but as to the spending 500*l.* in turning to some scientific profit the accumulated treasures there, *that* is a waste of public money, and an instance of *misdirected influence* on the part of the British Association. For you, gentlemen, will rejoice to hear, if any of you have not already heard it, and those who have heard it already will not grudge to hear it again, that through the influence of this Association, what Bessel wished, rather than hoped, is now in process of accomplishment: and that, under the care of the man who in England has done most to show how much may be done with an observatory, that national disgrace is to be removed, of ignorance or indifference about those scientific treasures which England has almost

unconsciously been long amassing, and which concern her as the country of Newton and the maritime nation of the world. For the spirit of exactness is diffusive, and so is the spirit of negligence. The closeness, indeed, of the existing agreement between the tables and the observations of astronomers is so great, that it cannot easily be conceived by persons unfamiliar with that science. No theory has ever had so brilliant a fortune, or ever so outrun experience, as the theory of gravitation has done. But if astronomers ever grow weary, and faintly turn back from the task which science and nature command, of constantly continuing to test even this great theory by observation, if they put any limit to the search, which nature has not put, or are content to leave any difference unaccounted for between the testimony of sense and the results of mathematical deduction, then will they not only become gradually negligent in the discharge of their other and more practical duties; and their observations themselves, and their nautical almanacs, will then degenerate instead of improving, to the peril of navies and of honour; but also they will have done what in them lay, to mutilate outward nature, and to rob the mind of its heritage. For, be we well assured that no such search as this, were it only after the smallest of those treasures which wave after wave may dash up on the shore of the ocean of truth, is ever unrewarded. And small as those five seconds may appear, which stir the mind of Bessel, and are to him a prophecy of some knowledge undiscovered, perhaps unimagined by man, we may remember that when Kepler was "feeling" as he said, "the walls of ignorance, ere yet he reached the brilliant gate of truth," he thus expressed himself respecting discrepancies which were not larger for the science of his time:—"These eight minutes of difference, which cannot be attributed to the errors of so exact an observer as Tycho, are about to give us the means of reforming the whole of astronomy." We indeed cannot dream that gravitation shall ever become obsolete; perhaps it is about to receive some new and striking confirmation; but Newton never held that the law of the inverse square was the only law of the action of body upon body; and the question is, whether some other law or mode of action, coexisting with this great and principal one, may not manifest some sensible effect in the heavens to the delicacy of modern observation, and especially of modern reduction. It was worthy of the British Association to interest themselves in such a subject: it was worthy of British rulers to accede promptly to such a request.

I have been drawn into too much length by the consideration of this instance of the external effects of our Association, to be able to do more than allude to the kindred instance of the publication of the ob-

servations on the tides in the port of Brest, which has, I am informed, been ordered by the French Government, at the request of M. Arago and the French Board of Longitudes, who were stimulated to make that request by a recommendation of the British Association at Edinburgh. Many other topics, also, connected with your progress and prospects, I must pass over, having occupied your time so long; and in particular I must waive what, indeed, is properly a subject for your general committee—the consideration whether anything can be done, or left undone, to increase still more the usefulness of this Association, and the respect and good will with which it is already regarded by the other institutions of this and of other countries. As an Irishman, and a native of Dublin, I may be suffered in conclusion to add my own to the many voices which welcome this goodly company of English, and Scottish, and foreign visitors to Ireland and to Dublin. We cannot, indeed, avoid regretting that many eminent persons, whose presence we should much enjoy, are not in this assembly; though not, we trust, in any case, from want of their good will or good opinion. Especially we must regret the absence of Sir David Brewster, who took so active a part in forming this association: but I am authorized, by a letter from himself, to mention that his absence proceeds entirely from private causes, and that they form the only reason why he is not here. Herschel, too, is absent; he has borne with him to another hemisphere his father's fame and his own; perhaps, from numbering the nebulæ invisible to northern eyes, he turns even now away to gaze upon some star, which we, too, can behold, and to be in spirit among us. And other names we miss; but great names, too, are here: enough to give assurance that in brilliance and useful effect, this Dublin meeting of the Association will not be inferior to former assemblings, but will realize our hopes and wishes, and not only give a new impulse to science, but also cement the kindly feeling which binds us all together already.

# THE STATE OF SCIENCE.

*Report on the Recent Progress and Present Condition of the Mathematical Theories of Electricity, Magnetism, and Heat. By the Rev.* W. WHEWELL, *Fellow and Tutor of Trinity College, Cambridge.*

THE trophies of Miltiades would not let Themistocles rest. The trophies of Newton, won at the end of the seventeenth century, made it impossible for the physical philosophers of the eighteenth not to attempt new victories in the application of mechanical principles to the phenomena of the material world. Newton himself had pointed out this as the business of his successors. "I have deduced," says he, at the end of his preface to the *Principia*, "the motions of the planets by mathematical reasoning from forces; and I would that we could derive the other phenomena of nature from mechanical principles by the same mode of reasoning. For many things move me, so that I somewhat suspect, that all such may depend on certain forces by which the particles of bodies, through causes not yet known, are either urged towards each other and cohere according to regular figures, or are repelled and recede from each other: and these forces being unknown, philosophers have hitherto made their attempts on nature in vain. But I hope that the principles here laid down may supply some light either to this mode of philosophizing or to some one which is more true."

It is usually assumed that Newton's anticipations and wishes have been fulfilled. Several mathematical and mechanical Sciences have since his time made their appearance in the world, claiming to be the younger sisters of Physical Astronomy; like her, fed by exact facts, formed by rigorous principles. Yet their birth and reception have never excited so much general notice as such events might be expected to produce; they have gradually become known to a limited circle of mathematicians, and have not,

like the first-born of their race, filled the civilized world with the noise of their fame. Moreover, it is allowed, or rather boasted, that it is only very recently that the mathematical train of reasoning which belongs to several of these new sciences has been rendered complete. It may therefore be of service to examine the claims of these parts of knowledge so far as they profess to be mathematical and mechanical sciences. I hope the undertaking will lose all appearance of presumption, when it is recollected that, in executing it, my main task will be, to study certain mathematical theories and calculations, to look at the recorded facts which are alleged to confirm these theories, and to describe as distinctly as possible the result of the comparison. Such an employment will not lead the writer to trespass on the domain of any one whose business is with new classes of facts, nor tempt him to judge any theory which does not profess to depend entirely on mathematical calculation from measured observations.

The Sciences to which I shall at present direct my attention are those of Electricity, Magnetism, and Heat. These sciences have sufficient connexion, both in the mathematical reasoning by which they have been established and the philosophical principles on which they depend, to make them a fit group to be treated of together. Though they have several features in common, I shall give a brief account of each separately.

*Electricity.*—Electricity, after being brought under distinct conceptions by Franklin and his contemporaries, was formed into a mathematical science by Æpinus; the theory of Æpinus was reformed by Coulomb; the calculations which Coulomb could not execute, Poisson in our own time has performed: such are the main steps in the history of electricity as a mathematical science.

The theory of electricity of Æpinus assumed one electric fluid only: it invested this fluid with these two properties, that its particles repelled each other with forces increasing with a diminution of their mutual distance; and that its particles attracted the particles of all other bodies with a force following the same law. On these suppositions (assuming also the difference of conductors and electrics with respect to the easy transfer of the fluid) a great part of the facts of electricity by induction, and of electrical attraction and repulsion, could be explained in a manner strictly mechanical.

But taking the whole of the experimental facts, a third supposition was found to be necessary;—that the particles of all bodies repel each other with the same force with which they attract the electric fluid. For, without this addition to the theory, how could two negatively electrified bodies repel each other with the same force as two positively electrified, since by supposition

they had both a deficiency of the repulsive element? Æpinus therefore found himself obliged to ascribe a mutual repulsion (incomparably greater than the force of gravity) to the particles of all bodies.

Coulomb established, what Mayer and Lambert had already ascertained, that the electrical force follows the law of the inverse square of the distance; and this was to be taken therefore as the law of the attraction and repulsion of the particles of the fluid in the Æpinian theory: but the theory stood in need of modification, and this it received from Coulomb.

Coulomb's reform of the electric theory of Æpinus consisted in assuming *two* opposite fluids, each attracting the particles of the other and repelling its own, by which means the repulsive force of the particles of the bodies was no longer admitted. The theory of one fluid had had many adherents, and the most persuasive argument in its favour was its greater simplicity; but when it was shown to involve the assumption that the particles of all bodies, though they attract each other with a force varying inversely as the square of the distance by the law of gravitation, repel each other by a much greater force varying according to the same law, the doctrine of a single fluid certainly lost at least the prerogative of simplicity. And when further it appeared that the same reasoning applied to magnetism; and that, on the hypothesis of one electric and one magnetic fluid the particles of iron must have, besides the attraction of gravitation, two other forces of mutual repulsion, one electric and one magnetic, all the three forces following the same law, it could not be doubted that the superiority of simplicity was transferred to the side of the hypothesis of two opposite electric and two opposite magnetic fluids. These hypotheses accordingly Coulomb adopted.

It now became necessary to calculate the results of the hypothesis; and so far as this went, the results of the Æpinian and Coulombian theories were the same. The calculation could be performed in a very limited range of cases, according to the mathematical methods which were in common use at that time. Æpinus had traced the general character of these results in his work published in 1759; and Cavendish, in the *Phil. Trans.* for 1771, had examined them further, assuming any law between the inverse simple power and inverse cube of the distance, but obviously inclining to the inverse square. But Coulomb had invented and employed delicate methods of ascertaining precisely the distribution of electric intensity in many cases not contemplated by preceding writers, and had to calculate the results of such cases for the sake of that comparison on which his theory was to rest.

The calculation in these cases was far from easy. The same entanglement occurred here which mathematicians had already *found so perplexing in the problem of the Figure of the Earth*; namely, that the attractions could not be calculated without knowing the form of the mass, and yet the form depended on the equilibrium of those very attractions. The only ways in which this difficulty could be surmounted were, either to devise methods for finding the attractions of bodies which should be applicable to all forms; or else, to assume some form, and to calculate the attraction, and then to modify the assumption so as to make it approach to a fulfilment of the conditions of equilibrium. The first-mentioned method was in the course of invention by Legendre and Laplace at the time of Coulomb's researches. Some of the earliest memoirs in which it is used appear in the very same volumes of the Transactions of the French Academy as Coulomb's memoirs on electricity and magnetism (1782—1789). But it required a long period to familiarize even the best mathematicians with this method, and its application to electricity was reserved for a later period, and for the skill of M. Poisson. In the mean time Coulomb applied, with great industry and ingenuity, such artifices as were obvious to a geometer of that time. For example*, in treating the case of two spheres, in order to determine the proportion in which the electric fluid distributes itself between them when one is electrised and brought into contact with the other, he supposed, as an approximation, the fluid to be uniformly spread over the surface of each, in order to calculate its attraction; although it is manifest that, in fact, the mutual repulsion of the parts of the fluid will make its density vanish at the point of contact, and increase gradually up to the point diametrically opposite. In order to correct this process, he supposes a small segment of the sphere surrounding the point of contact to be void of fluid, and finds the effect of this segment as if it were a circular plane. In a case in which a sphere is in contact with other spheres at two opposite points or *poles*, he finds the attraction of its fluid on two suppositions; one, that it is spread uniformly, another, that it is all collected in a ring at the *equator* of the sphere: the real distribution will be intermediate between these two suppositions, because the density of the fluid at the *poles* vanishes, and increases gradually up to the equator; hence Coulomb takes the mean of the results of the two suppositions, as more approximate to the truth than either of them. He pursues methods of this kind with a very unsparing expenditure of labour; for instance,

* *Acad. Paris.* 1787.

he calculates the relative quantities of electricity which will exist at the surfaces of each of 25 spheres, placed in a straight line, by elimination among the requisite number of equations*.

In this manner Coulomb obtains theoretical results corresponding to a great number of experiments carefully made and extremely varied in their circumstances. The agreement of the theoretical with the experimental numbers is not exact; which indeed could not be expected, since the former are only approximations, and the latter are affected by unavoidable errors. But an agreement appeared in the general scale and proportion of the numbers which shows that the theory gave in all cases the true quantity either exactly or very nearly; and this was further confirmed by the consideration of extreme cases, as pointed conductors, long conducting strings, and the like. Cases including long wires and plates had also been calculated by Cavendish, and found to agree in their general features with experiments. It should be observed also that Coulomb, at the very outset of his researches, had begun by ascertaining experimentally the formulæ by which allowance was to be made for two main causes of error, the dispersion of electricity from electrised bodies into the air, and its escape along the supports which were intended to insulate†. I conceive that if the results of the theory and of experiment, as stated in Coulomb's six memoirs on electricity‡, were collected in parallel columns, the amount of evidence would be considered as quite sufficient to prove that the theory gave at least good approximate *laws of the phenomena* in a large class of cases of the distribution of electricity; and adding to these the previous agreement obtained by Cavendish, this merit might be claimed for it through almost the whole range of obvious cases.

I do not think Coulomb has anywhere, after thus establishing the truth of his formulæ, summed up the evidence, and given his own view of the degree of certainty of the theory. In his 6th memoir on electricity§, he speaks of the two theories of one and of two electric fluids, and adds, "As these two explanations have only a greater or less degree of probability, I warn the reader, in order to put the following theory out of the reach of all systematic dispute, that in the supposition of two electric fluids I have no other intention than to present the results of calculation and of experiment with the smallest possible number of elements, and not to indicate the true cause of electricity. I shall reserve for the end of my labours on electricity the exa-

* *Acad. Par.* 1788, p. 641.

† 3rd memoir, *Acad. Par.* 1785, p. 612.

‡ *Acad. Par.* 1785—1788.

§ *Acad. Par.* 1788, p. 673.

mination of the principal systems to which electric phenomena have given birth."

Coulomb died in 1806, I believe without fulfilling the intention he here expresses. But we may allowably conjecture, I think, that the tenderness with which he here speaks of the theory of one fluid was not so much the expression of his own conviction as the effect of a wish not to shock the predominant persuasion on this subject, which from the time of Franklin had been in favour of one fluid only. We can hardly suppose Coulomb to have allowed this theory of one fluid more than a very minute comparative probability, not worth reckoning, when we recollect that he had so strongly pointed at the absurdity of the hypothesis which that theory necessarily involves, of the mutual repulsion of all the particles of matter.

Ever since the time of Newton, it has been customary for persons, attempting to pronounce judgement upon a philosophical theory, to refer to his "Rule of Philosophizing," "Causas rerum naturalium non plures admitti debere quam quæ et veræ sint, et earum phænomenis explicandis sufficiant." So far as the question of one or two fluids is concerned, it is to be recollected that we must at any rate have two *causes*, attraction and repulsion; and that, in fact, to attribute these forces to the fluids alone is to take *fewer kinds* of causation than to attribute them both to the particles of the fluids and of the body. Also in such estimations much stress has usually been laid upon the condition of the "vera causa". There is this very material difficulty in the application of that part of the rule, that it supposes us already to be in possession of the means of distinguishing true causes from untrue. If we really had such a criterion, by much the most important Rule of Philosophizing would be one in which the criterion should be stated. But if by a *vera causa* we mean (as it would seem men usually do mean) a cause already supposed to be known by mechanical effects; although the rule so understood appears to be very arbitrary, we may apply it to the theory of electricity. The existence of some electric fluid (whether one or two) as a true cause of the phenomena may, on this view, be held to be proved by the facts which suggested such a conception from the first;—by the accompaniments of the discharge,—the spark,—the sound; and especially the mechanical effects,—the shock, the power of striking, breaking and penetrating material objects. And thus the belief of one electric fluid at least is forced upon us as a physical truth, while the theory of two fluids rather than one is established by its being proved to involve, in reality, the simplest system of assumptions under which the phenomena can be explained.

The Coulombian theory of electricity had thus a fair claim to be considered as satisfactorily proved; and it was further confirmed, or countenanced at least, by the simultaneous and similar establishment of a parallel theory of magnetism. Yet this theory of electricity made its way but slowly to general acquaintance and acceptation. Here, as in physical astronomy, the length and complication of the mathematical calculations which the estimation of the theory presupposed, put it out of the reach of students in general; and the mathematical reasoning was not here, as in physical astronomy, invested with a kind of dignity by the grandeur of the cosmical views on which it bore.

The Æpinian theory was hardly known in England, except by name, till the late Prof. Robison gave a view of it, at considerable length, in the article ELECTRICITY in the *Encyclopædia Britannica*. In an appendix to this article the memoirs of Coulomb inserted in the *Acad. Par.* for 1786 and 1787 are referred to, but without any notice of the question of one or two fluids. The *Traité de Physique* of Haüy, published in 1803, and that of Biot in 1816, made the theory more generally known; but the latter date was subsequent to M. Poisson's important labours upon it, of which we must now give some account.

By using the methods invented by preceding analysts, in order to determine the figure of the planets, in the doctrine of universal gravitation, M. Poisson was enabled to solve exactly those problems respecting the distribution of the electric fluid on the surface of spheres which Coulomb, as we have seen, had been obliged to attack indirectly. The most material of the analytical improvements of which M. Poisson availed himself was the use of certain functions, possessing very curious properties, which have by recent writers sometimes been termed *Laplace's Coefficients*. These functions play so important a part in all the sciences which I have here to review, that it will be proper to give some account of them.

Suppose a thin stratum of variable thickness distributed upon a sphere symmetrically with regard to the axis,—thus, take, for instance, the protuberant part of the terrestrial spheroid,—and let it be proposed to find the attraction of this stratum upon a point anyhow situated, the position of the point being given by means of its angular distance from the north pole of the sphere, and its linear distance from the centre. The attraction on this point will then depend upon the position of the point, and upon the law of thickness of the stratum, by certain complex integrations. But this attraction may be resolved into a series of terms, each of which is a particular solution of the problem. Each of these terms contains two factors, one depending on the position of the

attracted point only, and the other on the corresponding thickness only; and this resolution, and the peculiar properties of these factors, it is which so much facilitate the treatment of attractions. These functions were introduced by Legendre, so far as regards their use for figures of revolution, and are thus employed in the *Savans Etrangers*, tom. x.* and in the *Mém. Acad. Par.* for 1784, read July 7, 1784, published 1787. In the *Mém. Acad. Par.* for 1782, published 1785, is a memoir by Laplace, in which the use of analogous functions is extended to figures not of revolution; and in consequence of this step, and of Laplace's frequent use of these functions, they have, as I have said, sometimes been designated by his name. So far, however, as they have been applied to the calculations of electrical theory by M. Poisson, those only have been used which apply to figures of revolution, which, as we have seen, are rather Legendre's functions than Laplace's. Mr. Ivory, in the *Philosophical Transactions* for 1812, treated their properties in a manner which was at once allowed by their French inventors to have given them a clearness quite new. Mr. Murphy in a separate work† has presented them and their application with great analytical elegance and simplicity. The most important parts of Poisson's application of these functions to the case now under notice may be found in part 2 of the article ELECTRICITY in the *Encyclopædia Metropolitana*.

By the use of these functions M. Poisson was enabled‡ to reduce the distribution of electricity upon two spheres which act on each other, to a certain functional equation. In the case where the spheres are in contact, he solved this equation by means of definite integrals, and thus obtained finite and exact formulæ for the density of the electricity at all the points of the two spheres, and for its whole quantity in each sphere. He had thus the means of much more rigorous comparison of Coulomb's experiments with his theory than Coulomb himself had been able to make.

The result of this new comparison was a confirmation of the inference from the old one; and especially in those cases where Coulomb's methods of calculation had been most inadequate, as in the instance of two spheres in contact. The experimental and theoretical numbers in fourteen such cases came very near each other; the mean error being $\frac{1}{30}$ of the quantities themselves if the positive and negative errors be allowed to balance

* According to Legendre's own reference, which I have not been able to verify.

† *Elementary Principles of the Theories of Electricity, Heat, and Molecular Actions:* Part I. Cambridge 1833.

‡ *Mém. Inst.* 1811.

each other, or less than $\frac{1}{12}$ of the quantities taking the mean of the absolute errors. Under the various chances of inaccuracy which belonged to the nature of the observations, this was as near a coincidence as could be expected.

M. Poisson's calculus also explained the electric spark which passes when an electrised body is brought very near another body; for it appeared that the thickness or density of the electric stratum on the nearest points of two spheres would approach to infinity, and consequently the tension would become infinite, as the spheres approach to contact. Before this contact, therefore, a spark passes from one to the other.

It will be recollected that the numerical coincidences of observation and calculation are in addition to the explanation of electrical attraction and repulsion, and of all the general phenomena of induction which the theory supplies, or rather which force some such theory upon us. It cannot be denied, then, that the Coulombian theory stands upon strong grounds so far as the *statical* phenomena are concerned. Still, it is always desirable that a philosophical theory should be confirmed and verified by observations and measures added to those on which it was originally founded; and it is to be remembered that the calculations of Poisson have been confined to spherical conductors. A true theory will commonly be upheld by a constant series of confirmatory experiments, made by students or professional observers, whether or not it is thought worth while to publish the results of such trials. I do not know that many such *measures* of the phenomena of statical electricity have been taken. Among the most important of such measures are those made by Mr. Snow Harris, which were exhibited before a section of the British Association at Cambridge in 1833, and are described in the Transactions of the Association for that year, p. 386. These experiments have since been more fully detailed in a paper printed in the *Philosophical Transactions* for 1834, Part II. p. 213. His invention of a mode of measuring the quantity of electricity by what he calls a *unit jar*, is valuable as offering a new mode of verifying, or at least testing, the general laws of electrical action; and the considerable amount of the statical forces which were brought into action in his experiments inspires more confidence, at least at first sight, than the extremely minute quantities employed by Coulomb. His forces were in many cases measured directly in grains. The detailed comparison of the results with experience would be a matter of some labour, and indeed of some difficulty; for in some cases circumstances which would affect the result are not stated, and the electric fluid was distributed through conductors so complex in their form that it would not

be easy to trace by calculation the consequences of the theory. Even what might appear very simple problems, as the distribution of the fluid on rectangular plates, have not, so far as I am aware, been solved mathematically, and therefore we cannot immediately compare Mr. Harris's experimental results in such cases with the theory. But we may observe a general difference in the mode of measuring the intensity of the action in these experiments from those of Coulomb. In Coulomb's apparatus the energy of the electric action was estimated by the mutual force which is exerted between two *insulated* particles; in Mr. Harris's researches the conducting body, whose intensity is to be examined, and the other conductor which it attracts or repels, are, one or both of them, uninsulated, or at least connected with a large extraneous conducting surface. This arrangement would make peculiar calculations requisite. In their more obvious results, however, Mr. Harris's experiments confirm the Coulombian theory: thus, it is shown* that the force of electrical attraction is inversely as the square of the distance. Also the rule which Mr. Harris obtained, and which appears to have surprised him, that the *intensity* of the force measured in his way is as the *square of the quantity* of electricity, is a consequence of the effect of induction on the uninsulated conductor. It might be well worth while for some new Poisson to examine the rest of Mr. Harris's results in their bearing upon the Coulombian theory; although, as has been said, there would be considerable mathematical difficulties in the course of such a comparison.

*Magnetism*.—If we now pass to the consideration of Magnetism, we have, to a considerable extent, to repeat the same story which we have had to tell respecting Electricity. The attractions and repulsions of magnets, their polarity, the transient magnetism of soft iron, led to the assumption of magnetic fluids, one, acting in excess or defect, or two, an austral and a boreal, moveable in soft iron, but fixed in hard steel; and thus, as in the case of electricity, the facts of attraction, repulsion, induced magnetism, could be pretty completely represented. Æpinus worked out mathematically the consequences of the assumption of one fluid, in the same work in which he performed the same part for electricity†. Coulomb reformed this theory, having in the first place ‡ established that the force of the particles of the fluid is inversely as the square of the distance; a law which Lambert and Mayer § had already discovered, and which was subsequently confirmed by Barlow and Hansteen.

* *Phil. Trans.* 1834. Part II. p. 238.

† *Tentamen Theoriæ Electricitatis et Magnetismi.* 1759.

‡ *Acad. Par.* 1784.

§ *Biogr. Univ.*, art. Coulomb.

Coulomb reformed the Æpinian theory of magnetism, as he reformed that of electricity, by adopting the supposition of two fluids instead of one; and for the same reason, namely, that two south poles and two north poles alike repel each other; which could not take place if either austral or boreal magnetism were a mere negation, without supposing a mutual repulsion common to all magnetic matter. But there was another more peculiar hypothesis which he found it necessary to introduce in addition to those of Æpinus. Several conductors placed in contact, end to end, make one conductor; but several pieces of iron so placed do not make one magnet; each piece has its own poles. If we cut a magnet in pieces, each piece has polarity. These facts forbade the supposition that the fluids which give magnetic polarity are transferred from one part of the iron to another; yet these fluids must be separated to produce the phænomena. Coulomb reconciles these conditions by supposing that the magnetic body consists of small particles; and that the fluids are separated in each such particle, but never pass out of it. He shows that in this way a line of particles would have a sensible magnetism at each point, arising from the excess of one magnetism at such point over the opposite magnetism; and he proves that* "on this hypothesis the calculation of the magnetic actions, or of the intensity of the magnetic forces of each point, must give us precisely the same result as that of the transport of the magnetic fluid from one extremity of a needle to another."

His calculations for the confirmation of this theory of magnetism are therefore the same as those which had been requisite for the theory of electricity. He found such a conformity between the fundamental experiments and the calculation, as gave, in his opinion, great weight to the system†. Thus he ascertained by experiment, that in two similar saturated needles of the same substance, the moments of the force of terrestrial magnetism were as the cubes of the homologous dimensions, which agrees with the theory. He also found by experiment that the magnetic intensity at different points in a long needle was nearly as the distance from the centre; and by conceiving the needle as a cylinder divided into portions, and calculating the mutual action of these portions‡, he approximately verified this as the theoretic law: and he asserts generally, that§ "by the help of certain corrections, it is easy to make the theory square with magnetic phænomena."

Probably few persons who have studied the subject since that time have been disposed to deny that this theory gives the *laws*

* *Acad. Par.* 1789, p. 492. † *Ibid.*, p. 487.
‡ *Ibid.*, p. 485. § *Ibid.*, p. 492.

*of the phenomena*, although there did not exist till lately so correct and pertinent a collection of measures in support of the magnetic as of the electric theory. Indeed, the peculiar feature in the case of magnetism, that the fluid cannot be transferred from one body to another, prevented measures of the same kind, or of the same accuracy, being employed. And the same circumstance affected the theory more directly, in that it left the theorist without any steady conviction of the real existence of the fluids which were the subject of his calculations. There was no magnetic discharge analogous to the electric discharge; and thus the magnetic fluid was a hypothesis resting for its evidence only upon the one class of facts for which it accounted. More recently, indeed, we have had the magnetic fluid proved to be real, and connected with the electric fluid by most curious and unforeseen relations, but of these it is not our business now to speak. The Coulombian theory of magnetism is still an important portion of science, as claiming to give the laws of the statical phenomena: in what manner these laws result from the best views we can obtain of the cause of the facts is a matter for subsequent consideration.

The next important event in the history of the theory was a series of good observations, made at first with no reference to the theory. In the year 1819 Mr. Barlow undertook a course of magnetic experiments with a view to enable himself to correct the local attraction exerted by the iron which ships contain upon the compass. In the course of these researches "he discovered," as he says*, "certain magnetic laws which seemed to him likely to pave the way to a mathematical theory of magnetism;" a mode of expression which seems to show that Coulomb's theory had obtained little currency. Mr. Barlow's experiments were made at first by measuring the deviation produced in a compass needle by an iron sphere. The result was, that, for such a sphere, there exists a *plane of no attraction* coincident with the magnetic equator; and if we measure the magnetic latitude of the compass from this plane, and the magnetic longitude from the east and west points, the tangent of the deviation produced by the attraction of the sphere is as the sine of the double latitude, and as the cosine of the longitude: it is also as the cube of the diameter of the sphere directly, and as the cube of the distance of the compass from the centre inversely.

These rules were discovered empirically, and published in 1820; and shortly afterwards Mr. C. Bonnycastle undertook to deduce these laws from a theory analogous to Coulomb's the-

* *Magnetic Attractions*, Preface, p. 1.

ory of electricity, treated as M. Poisson had treated it in 1811. Mr. Bonnycastle found that all Mr. Barlow's results agreed with such a theory.

But a peculiar circumstance in the experiments attracted Mr. Barlow's notice, and made him imagine that the theory required modification. He found that the attraction of a solid iron sphere was the same as that of a hollow shell, even when the shell was thin; and he was led to believe that the magnetic power of iron resides wholly in the surface. This result was confirmed, as to the facts, by Capt. Kater*. Mr. Barlow considered this result as inconsistent with the theory of Coulomb, in which the magnetic fluids in every particle of the mass were supposed to be dislodged by the action of a neighbouring magnet.

Yet a little attention shows us that this is in fact a consequence of the Coulombian theory. I have already (p. 11) quoted the passage in Coulomb's memoir on magnetism in which he asserts that the distribution of the sensible magnetism will be the same as if the fluids were transferrible from one part of the body to the other. Now it is easily shown that on this supposition all the sensible magnetism is repelled to the surface, as all the sensible electricity is, according to the parallel theory of electricity, and as Coulomb had shown that it is in fact. It is true, that though the superficial disposition of magnetism followed from the theory, and was involved in the general proposition above quoted, I do not know that Coulomb anywhere expressly asserts the fact respecting magnetic bodies, or that he made any experiments to confirm it. Yet it may be observed, that in his second memoir on electricity and magnetism † he proved that in a long needle the magnetic force may be conceived to be collected very near each end, which is an indication of the same kind of effect of the theoretical properties of magnetism.

It was probably the experimental labours of the English philosophers which led M. Poisson to perform the same office for the magnetic which he had executed so well for the electric theory;—to trace the consequences of Coulomb's hypotheses by the aid of powerful and general analytical methods. In February 1824 a memoir of his upon this subject was read to the Institute, and published in 1826, in the memoirs *for* 1821 and 1822, according to the strange method of publication of the French Academy. In this memoir he obtains expressions for the attractions and repulsions of a body magnetised by influence upon any point, and examines in particular the case in which the body is a sphere. M. Poisson gives the name of *magnetic*

* See his memoir in the *Phil. Trans.* 1821. † *Acad. Par.* 1785, p. 578.

*elements* to the small parts of bodies within which the magnetic fluids can be separated. Supposing these elements to be spherical, he would be enabled to determine the conditions of equilibrium of the free magnetic fluid at the surface of each element, by the same analysis as in the case of electricity; using, throughout his researches, those peculiar functions which we have termed *Laplace's coefficients, and which introduce such extraordinary* facilities into researches of this kind. It further appears, from the nature of the equations (p. 283), that we need not know the form of these elements; for the form of the elements, and the proportion of their sum to the whole mass of the body, enter into the result jointly, so that we do not trace the separate effect of these data.

M. Poisson therefore (p. 290) takes the equation of equilibrium on the supposition that the magnetic elements are spherical; and he then finds (p. 306) that this equation coincides with the condition of equilibrium for electricity, on the supposition that the sum of the magnetic elements is equal to the mass of the body (*i. e.* in his notation, $k = 1$). And in general (p. 303), the magnetic action of a body of any form is equivalent to that of a thin stratum of magnetic fluid at the surface, although the fluids are separated in every part of the mass. In a subsequent part of his memoir, M. Poisson applies his conclusions to determine the distribution of magnetic fluid in a solid or hollow sphere acted upon by the terrestrial magnetism. He refers to Mr. Barlow's experiments, and to his inference that magnetism resides at the surface alone; he observes that the inference is not warranted, and that the only conclusion which we are justified in drawing by the fact, as compared with the formulæ, is that the sum of the magnetic elements is equal exactly, or very nearly, to the whole mass of the body.

The force exerted by a body in which magnetism is induced, is a joint result of the distribution of the magnetism thus excited, and of the position of the point acted on. The verification of M. Poisson's theory would require experiments made with masses of iron of various forms, as well as measures of the effect on a needle in various situations with reference to the mass; and the theory, thus verified, would disclose to us the distribution of the magnetism at the surface of iron under given circumstances.

The verification, with respect to the position of the point acted on, has been executed to a satisfactory extent. M. Poisson observes (p. 336) "that the laws of the deviation of compass needles are in accordance, whether we deduce them from theory, or from observation" as Mr. Barlow had done; "and thus that gentle-

man's numerous observations are a remarkable confirmation of the theory of magnetism here presented." I do not think it necessary to dwell upon the necessity of a correction for the length of the needle, and its magnetic effect upon the iron sphere, which M. Poisson conceives to be requisite in calculating Mr. Barlow's experiments. But it may be observed, that M. Poisson's assumption that his quantity $k$ (which expresses the ratio of the sum of the magnetic elements of the body, to the sum of all its parts,) cannot exceed unity, does not appear to be incontestable, since it involves the supposition that the whole magnetic attraction or repulsion of each such element is the same as if its form were spherical, which supposition is introduced p. 290 of the memoir.

The same volume of the Memoirs of the Institute contains a second memoir of M. Poisson on the same subject, read December 27, 1824. In this the author observes, that though Mr. Barlow's observations afford an important confirmation of the theory, it was desirable that it should be subjected to trials of a more varied kind. On returning to his formulæ, he found that they could be very simply solved for the case of any ellipsoid whatever. Now a very flat ellipsoid may approach indefinitely near to an elliptical or circular plate; a very slender ellipsoid may approach indefinitely to a linear bar. Thus the mathematical theory of certain very obvious and extensive cases was attainable. We do not, however, possess any comparison of experiments with the formulæ thus obtained; and thus the verification of M. Poisson's theory, so far as the distribution of magnetism depends on the form of the mass of iron, is hitherto incomplete. M. Hansteen of Copenhagen, whose valuable work on terrestrial magnetism was published in German in 1819, had inferred from his own experiments, that in a linear magnet the magnetic intensity follows the law of the square of the distance from the middle point more nearly than any other power of that distance, a conclusion different from Coulomb's, as we have seen (p. 11)*.

It appears, however, to have been taken for granted, after the verification of the theory by Mr. Barlow's experiments, that it might be considered as established, and that mathematical methods of deduction might for the future be used, not to confirm the truth of its principles, but to apply them to any requisite purposes.

The most remarkable and important example of a "Deductio ad Praxin" of this kind, was that which Mr. Barlow made in the

* Hansteen, *Magnetismus*, chap. v. p. 165.

case which had first given rise to his researches, the correction of the deviations of ship-compasses produced by the "local attraction"; and it was this case which suggested to M. Poisson some of the problems of his second memoir. Mr. Barlow was soon enabled to perceive, from his own experiments, that the guns and other iron of a vessel produce the same effect as a small sphere of iron in a certain position; and his first idea was to place another iron ball on the opposite side of the compass, so as to *counteract* this effect. But when a ship moves into various positions, she turns round a vertical axis, which does not coincide with the axis of magnetic position. Therefore the relative magnetic situation of the disturbing and the correcting masses would vary with the changes of position of the vessel; and the correcting ball, in order to discharge its office, must be altered in place or size when the vessel turned its head different ways, an inconvenience which rendered this device almost nugatory.

He then proposed to place the ball in a certain fixed position, in which it would *double* the deviation arising from the local attraction; and finally, when he discovered that the attracting power of iron resided in the surface, he substituted an iron plate for the ball, and thus his apparatus and the mode of using it became convenient and easily managed.

The correcting plate so employed would produce the requisite effect if the attraction of the iron in the ship could always be referred to the same virtual centre. The attraction of a mass, however irregular, is equivalent to a single force acting to a single point or "focus of attraction". But this focus may be different in different positions of the irregular mass; for the magnetism which is developed by the earth's action in any mass will depend upon the form and position of its surface; and when the position varies, the position of the resulting attraction with respect to the mass may also vary. Hence, when a ship's compass is disturbed by the action of the irregular mass of iron which the vessel contains, it may happen that the same plate or ball, in the same relative situation, cannot either counteract or double the ship's attraction in all positions. Whether such effects are possible or not must depend upon calculation. By M. Poisson's investigations it appears that this possibility depends on certain conditions, and that it does not exist generally*. But it may be observed that the effect of the attraction of the vessel is greatest (and therefore the necessity of correction greatest) when the dip is considerable, because then the horizontal directive force of terrestrial magnetism is small. Now

* *Mémoire*, 1822, p. 531.

in such cases the disturbing masses, which assume their different positions by being turned round a vertical axis, will be nearly in the same magnetical attitude in all their changes, and therefore their effect will not much be altered. Thus Mr. Barlow's correction will be nearly complete in those cases in which it is most important. Mr. Barlow informs me, that in the voyages recently made towards the north pole, advantage was taken of the near coincidence of the magnetic equator with the horizon; and the contrivance of a *counteracting* plate, which had properly been rejected in other cases for the reasons just mentioned, was adopted with great success.

I do not consider it to belong to my present purpose to notice those experimental inquiries concerning magnetism which have not yet been brought into manifest connexion with the theory. One of the most important branches of the subject, that which has to do with Terrestrial Magnetism, has already been the subject of a Report presented to the Association by Prof. Christie, and since published*, and of a supplementary Report by Capt. Sabine in the present volume. I proceed therefore to another of the subjects which are included in my present task.

*Heat.*—The doctrine of the Conduction and Radiation of Heat, mathematically treated, is a subject which has excited considerable notice of late years; and its history brings before us several important questions of physics and mathematics. I will speak in order: 1st, of the Experimental Evidence of the Principles of this doctrine; 2ndly, of certain Difficulties which affect the Fundamental Equations; 3rdly, of the Mathematical Processes by which these equations have been treated; and 4thly, of the Application of the Mathematical results to several subjects of speculation.

1. *Experimental Thermotical Principles.*—The first step in the application of mathematical principles to conducted and radiated heat was made in the *Principia.* "It was in the destiny of that great work," says Fourier, "to exhibit, or at least to indicate, the causes of the principal phænomena of the universe." Newton assumed, as a simple rule evidently agreeable to facts, that the rate at which a body parts with its heat is proportional to the excess of heat; and on this assumption he rested the verification of his scale of temperatures. It is an easy deduction from this law, that if times of cooling be taken in arithmetical progression, the heat will decrease in geometrical progression. Kraft, and after him Richman, tried to verify this law by direct experiments on the cooling of vessels of warm water; and from

* *Report of Third Meeting*, p. 105.

these experiments, which have since been repeated by others, it appears, that for differences of temperature which do not exceed 50° centigrade, this geometrical progression represents the process of cooling with tolerable accuracy; not, however, with complete exactness, for it is found that at higher temperatures the cooling really takes place faster than the rate this law would assign.

The processes of the communication of heat to a surrounding medium, and to bodies in contact, have obviously much in common; and it was assumed that the rate of *conduction*, as well as the rate of *cooling*, are proportional to the excess of temperature. Philosophers were naturally led to endeavour to explain or illustrate this process by some physical notions. Lambert in 1755 * published an "Essay on the Force of Heat," in which he compares the communication of heat to the flow of a fluid out of one vessel into another by excess of pressure, and mathematically deduces the laws of the process on this ground. But the general facts of radiation, which soon after came into notice, modified this view, since it appeared that cold might be radiated as well as heat.

The doctrine of radiation was put in a simple and satisfactory form by Pierre Prevost of Geneva, about 1790; in which form it is often called the *Theory of Exchanges*; its leading principle being, that all bodies are perpetually exchanging their heat with one another by radiation. The mathematical reasoning upon the subject, of which we shall shortly have to speak, by Fourier, Laplace, and others, proceeded upon the assumption of the truth of Newton's law, that the rate of communication of heat, both in conduction and radiation, varies as the excess of heat. This is so far an approximation to the truth, that various experiments, made with a view to verify the theory, gave satisfactory results. Thus Biot †, by heating a long metallic bar at one end, found that the heights of thermometers, placed at equal intervals along it, followed a decreasing geometrical progression, as by mathematical reasoning from the theory it appears they ought to do. And in 1808, when Fourier had deduced from his formulæ certain peculiar relations of temperature when heat is propagated in an *armil* or ring, he made experiments which agreed with the calculation, and thus confirmed the theory, at least approximately. The whole mathematical doctrine of heat, as hitherto treated, has been founded on the truth of the Newtonian law thus verified.

Yet we now know that this law is not exactly true. At an

* *Act. Helvet.*, tom. ii. p. 172. † *Traité de Physique*, tom. iv. p. 671.

early period it had been noticed, as we have said, that the rate of cooling at high temperatures is faster than the theoretical rule. In 1817 the rule was reformed by MM. Dulong and Petit, whose investigations on this subject are an admirable example both of laborious experiment and of sagacious induction. Without dwelling upon the steps of their process, we may observe that they were led to this formula for the rate of cooling, $m\,a^{\theta}\,(a^{t}-1)$, where $\theta$ is the temperature of the surrounding space, and $t$ the excess of temperature of the body. This formula shows that the quickness of cooling for a constant excess of temperature is not constant, but increases in geometrical progression when the temperature of the surrounding space increases in arithmetical progression. From this rule, and from the theory of exchanges which makes part of their reasoning, MM. Dulong and Petit find that the quickness of cooling, so far as it depends on the temperature of the hot body, increases as the terms of a geometrical progression *diminished by a constant number*, when the temperature of the hot body increases in arithmetical progression. This explains the deviations previously observed, and gives a complete rule, remarkable for its symmetrical character*.

This correction of Newton's law will materially affect the mathematical calculations belonging to the subject; but probably the general features of the results will be the same as on the old supposition. M. Libri, an Italian mathematician, is the only person, so far as I am aware, who has applied Dulong and Petit's law to calculations of this kind. With this law for his basis, he has undertaken the problem of the armil, in a memoir read to the Institute of France in 1825, and since published at Florence†.

The application of mathematics to the problem of the communication of heat, requires not only the fundamental law of such communication to be given by experiment, but also certain numerical quantities which are different for different substances, and which express the specific power of conduction and of radiation for each substance. These quantities have been called by Fourier *conductibilité* or *conducibilité* extérieure et intérieure. Such terms are obviously improper, except we could apply the adjectives *conductible* or *conducible* to the substances, which it would be a gross solecism to do; but we may say of substances, that they are more or less conduc*tive*, and we may therefore properly speak of their exterior and interior *conductivity*.

2. *Fundamental Mathematical Formulæ.*—Supposing New-

* The temperatures in MM. Dulong and Petit's formulæ are those of the *air* thermometer.

† *Mém. de Math. et de Phys.*, 1829.

ton's law to be true, and the *conductivities* of the substance in question to be given, the determination of the progress of heating and cooling will involve mathematical relations and calculations, which, as we have seen, had begun to attract attention in the middle of the last century. But it was not till the beginning of this, that such problems were taken up with due generality, and followed into special consequences*. In December 1807 a memoir of Fourier's was read at the Institute, which must be considered as the commencement of a new mode of treating the subject. This memoir was published in 1808 in the *Bulletin des Sciences* of the Philomathic Society†; and in it was given the general partial differential equation between $v$, the temperature at a point of any substance of which the coordinates are $x$, $y$, $z$, and $t$ the time; namely,

$$\frac{dv}{dt} = a\left(\frac{d^2 v}{dx^2} + \frac{d^2 v}{dy^2} + \frac{d^2 v}{dz^2}\right):$$

this, with the equations which belong to the surface and express the conditions of exterior conductivity, contains the solution of the problem; though it was only in a few cases, and by means of refined analytical artifices, that the integrals of these equations could be obtained.

Problems concerning the motion of heat now drew the attention of the mathematicians of France; and the solutions of which Fourier was known to be in possession awoke the activity of other mathematicians. In the Memoirs of the Institute for 1809 (published in 1810), it is proposed, as the prize-question for 1812 (p. 96), "To give the mathematical theory of the propagation of heat, and to compare this theory with exact observations." Fourier's memoir was sent Sept. 28, 1811, and the prize was adjudged to it, probably as had been expected on all hands, in the ensuing January. This memoir, founded upon the one written in 1807, crowned in 1812, was not published till 1824, in the Memoirs of the Institute for 1819 and 1820; another remarkable example of the delay and ambiguity of date in French publications of this kind. While Fourier's memoir thus remained in the archives of the Institute, it was consulted by Poisson and Cauchy; in the *Bulletin des Sciences* for 1820 was published an extract

* "The form of the equation (the differential equation of the temperature of a bar which is come to a permanent state), and the form of the partial differential equation, which obtains when the bar grows hotter or colder, were indicated by M. Biot in 1801, in the extract of a memoir on the propagation of heat (*Biblioth. Britann.*, t. xxvii.) M. Biot deduces his equation from Newton's principle, applied to thin contiguous slices, integrates for the permanent condition, and verifies the result by his own experiments and those of Rumford. (Poisson, *Theo. de la Chal.*, 1835, p. 1.)

† tom. i. p. 112.

of a memoir of Fourier's on the Cooling of the Earth; Fourier's "Theorie de la Chaleur" appeared as a separate work with the date 1822, not containing, however, his investigations on the cooling of the earth. Notices of various results of the labours of Fourier and others appeared also from time to time in the *Annales de Chimie et de Physique*.

When Biot and Laplace turned their attention to this subject, they conceived that they saw a difficulty in the reduction of the question to a mathematical form, which difficulty Laplace thus states in the Memoirs of the Institute for 1809 (1810): "The quantities of heat received and communicated in an instant by any elementary slice of a conducting solid, must be infinitely small quantities, because the excess of temperature of each slice over the next is infinitely small; therefore the excess of the heat received over the heat lost will be an infinitely small quantity of the second order; and therefore the accumulation in a finite time will not be finite." "This difficulty," Laplace says at the period of which we speak, "has not yet been solved. Mathematicians often get true equations from false suppositions; they have done so in this case in supposing heat communicated by contact. Fourier's equations are right, but the true bases of them are to be found in the doctrine of the action of molecules *ad distans*."

Laplace's solution of this difficulty is, that we are to conceive each particle of a body receiving its heat, not from the particle immediately adjacent only, but from all the particles within its reach, the law of action diminishing rapidly as the distance increases. And he connects with this observation a series of remarks on the various classes of phenomena which may thus be reduced to molecular action; among which he mentions capillary attraction, electric and magnetic phænomena, the properties of elastic bodies, and finally the laws of heat. All these, he says, ought to be treated as cases of systems of discrete molecules, attracting and repelling each other at a distance.

That by considering fluid or solid bodies as composed of distinct particles, and by suitably assuming the forces which these particles exert on each other, we may represent their mechanical condition, and trace its consequences, is undoubtedly true. But it would be to go too far, to assert, on this account alone, that the only true conception of the physical structure of bodies is that which represents them as so constituted. This would be to mistake the use of the differential calculus for the evidence of a physical truth. Whether a comparison of special results of the molecular hypothesis with facts, will give it any countenance as the real state of things, is another and a very curious question, which

we may hereafter consider. But we may venture to say, that when Laplace, at the period of which we speak, asserted his own reasoning to be the only real basis of Fourier's equations, he took a partial view of the question. Fourier was not bound to take Laplace's solution of the difficulty, if, in his mode of reasoning, the difficulty did not occur, which was really the case. Fourier maintained that the quantity of heat transferred from one slice to the next in unit of time was a finite quantity, independently of molecular reasoning. For, as he showed, the quantity of heat transferred from one side of a slice to the other, is not only as the difference of temperatures of the two sides directly, but as their distance inversely: and when, in consequence of the evanescent thickness of the slice, one of those quantities vanishes, the other does so too, and the flow of heat remains expressed by a finite quantity; or, to take the matter in another form, if a bar of iron, one end of which is kept constantly hot and the other cold, be supposed to have acquired a permanent state of temperature in all its parts, there is a flow of heat from the hot to the cold end; and the quantity which passes through any section of the bar is equal to the quantity which in the same time is radiated from the whole of the colder surface beyond that section, and is therefore finite. Fourier's reasoning no more requires the introduction of molecular action, than do the reasonings by which the common formulæ of Hydrostatics (formulæ much resembling those of Fourier) are established in Mechanical Treatises.

But there are other circumstances bearing upon this question which well deserve to be considered. Fourier's reasonings apply to radiated as well as to conducted heat; and radiation is governed by peculiar laws, which may throw additional light on that kind of molecular action. There are, in particular, two laws, discovered by experiment, to which the theory must conform itself. One of these is founded on general and obvious experience,—that all bodies placed in an inclosed space assume, in the course of some time, the temperature of the inclosure; the other was established by special experiments by Leslie*. It is this :—that heat is emitted from every point of the surface of a hot body in all directions, and that the intensity of the heating ray in any direction is *as the sine of the angle* which it makes with the surface.

Fourier's theoretical explanation of these two laws must be looked upon as happy and successful; for he has shown that the same suppositions are requisite to explain the former general and simple fact, as to give the latter less obvious rule. The *law*

* *Experimental Inquiry into the Nature and Propagation of Heat*, 1804.

*of the sines* is requisite, in order that neighbouring bodies may assume the same temperature. The first assertion of this connexion appears to have excited some surprise in Paris, yet it is easily demonstrable *. It was announced as a curious result of Fourier's investigations, that if the law of the sines did not obtain in the radiation of heat from a surface, a particle would not necessarily assume the temperature of the inclosure in which it is contained:—that its temperature would depend upon its position; and within a shell of ice we should have at certain points the temperature of boiling water and of melting iron, arising from radiation alone. Perhaps this may become less apparently strange by attention to the following reasoning. The equilibrium and identity of temperature, between an including shell and an included body, cannot obtain upon the whole in every case, except it obtain between each pair of parts, taken on the surface of the body and of the shell respectively; that is, any part of the one surface, in its exchanges with any part of the other surface, must give and receive the same quantity of heat. Now the quantity exchanged, so far as it depends on the receiving surface, will, by geometry, be proportional to the sine of the obliquity of that surface; and as each surface may, in the exchange, be considered as receiving, the quantity transferred must be proportional to the sines of the two obliquities, that is, to that of the giving as well as the receiving surface.

But though the law of the sines is thus manifestly true, we have still to ask what is the physical ground of it? To this question also Fourier offers a reply. It arises, he says, from this: that the radiation takes place not from the surface alone of the body, but also from particles situated within a certain small depth of the surface. It is easy to see that on this supposition a ray emitted obliquely from an internal particle will be less intense than one sent forth from the same particle perpendicular to the surface, because the former will be intercepted in a greater degree, having a greater length of path within the body; and Fourier shows that whatever be the law of this intercepting power, the result will be, that the radiative intensity is as the sine of the angle made by the ray with the surface.

Thus Fourier's theory of molecular *extra-radiation* acquires, to say the least of it, great consistency. But this cannot be considered, I think, a sufficient ground for the hypothesis maintained by Laplace and by M. Poisson, that conduction also takes place by *intra-radiation*, or that we *must* conceive bodies as composed of distant particles radiating upon each other.

* *Ann. Chim.* iv. p. 129, 1817.

But without further examining this point, or the general question of the reality of the molecular hypothesis, which depends mainly on the same considerations, I shall proceed to the next division of the subject.

3. *Mathematical Solutions of the Equations.*—In the same memoirs of Fourier in which he gave the differential equations for the motion of heat in various cases, he also gave the integrals of these equations in some of the most important instances. These solutions were obtained by means of very peculiar artifices and have led to, or been connected with, some remarkable disquisitions on points of pure analysis. It does not belong to my purpose to give any account of the labours of writers on heat in this point of view; but in order to bring under the reader's notice some of the leading features of the inquiry, I will briefly refer to two of the problems, the motion of heat in a rectangular plate, and in a solid sphere.

The first of the special problems treated by Fourier* is to find the ultimate and permanent distribution of heat in a rectangular lamina, of which one side is kept uniformly hot, the two adjacent ones uniformly cold, and the fourth side is at an indefinite distance. The equation belonging to this case is of a simple and well-known form†; and it is easy to obtain a possible solution of it‡, which exhibits sufficiently the general course of the phenomena; namely, that in proceeding along the lamina from the hot end, the temperature diminishes in geometrical progression at equal distances; but that in proceeding across the lamina from the middle to each cold side, the temperature diminishes according to the law of a cosine. This possible case is, however, merely a particular solution; and in order to make the solution complete, we must be able to extend it so that the temperature of the boundaries of the lamina shall be regulated by any prescribed law; for example, so that at the hot end it shall be uniform from one side to the other, instead of diminishing from the middle each way, as it would in the case just stated. And this consideration introduces us to a very remarkable province of analysis; for it is easy, by adding together any number of such particular solutions as we have mentioned, to produce a function which gives a more general solution §; but in order to satisfy the condition just stated, this function must

* *Theorie de la Chaleur.*

† $\frac{d^2 v}{d x^2} + \frac{d^2 v}{d y^2} =$; 0 where $x$ is parallel to the cold sides, $y$ to the hot one.

‡ $v = \epsilon^{-mx} \cos m y$.

§ $v = a \epsilon^{-mx} \cos m y + a' \epsilon^{-m'x} \cos m' y + a'' \epsilon^{-m''x} \cos m'' y +$, &c.

be *discontinuous*, and we are thus led to that curious and perplexing part of analysis which treats of such functions. It is the less necessary for me to dwell on this train of investigation, in as much as it has been fully treated of by Mr. Peacock in his Report on the Progress of Analysis, read last year*. I will only observe, that the general solution † is in this case expressed by an infinite series of such terms as I have mentioned, the series having such coefficients that, at the extremity of the bar, the function represented by it is discontinuous.

In the case of the lamina just spoken of, we have two dimensions of space to consider (length and breadth, the thickness being left out of view); but we have nothing to do with the time, because we consider only the ultimate and permanent condition of the body. Another of the problems treated by Fourier, that of the distribution of heat in a sphere, is simplified in a different way. By supposing the heat to be uniformly distributed about the centre, the temperature at any point depends only upon the distance from the centre and the time; and the differential equation which expresses this dependence may be obtained ‡. But in this case we have necessarily a second differential equation §, which expresses the conditions of radiation at the surface, as the first expresses the conditions of conduction in the interior. In this case also we can easily assign a particular solution ‖; or a possible distribution of the heat and a possible relation of the conductive and radiative powers, which shall cause the cooling of the sphere to follow a certain law with respect to the times; namely, that the temperature shall diminish in geometrical progression for equal increments of time. The extending this particular into a general solution consists, in this as in the former case, in adding together a number of simple terms of this kind, and in so determining them that they shall agree with the given relation of conductive and radiative powers of the globe ¶, and also that the original distribution of heat shall be

* *Report of Third Meeting*, p. 251, *et seq.*

† $\frac{\pi v}{4} = \varepsilon^{-x} \cos y - \frac{1}{3} \varepsilon^{-3x} \cos 3y + \frac{1}{5} \varepsilon^{-5x} \cos 5y - \&c.$—*Théorie de la Chaleur*, p. 190.

‡ $\frac{dv}{dt} = k\left(\frac{d^2 v}{dx^2} + \frac{2}{x}\frac{dv}{dx}\right).$

§ $\frac{dv}{dx} + hv = 0$ when $x = X$, X being the whole radius.

‖ $v = A\frac{\sin(nx)}{x}\varepsilon^{-knt^2}.$

¶ $nX = \left(1 - \frac{h}{k}X\right)\tan nX$, which determines $n$.

such as is given. The latter condition may again carry us among discontinuous functions. But the solution enables us to see that each concentric spherical shell of which the globe is composed will diminish in temperature in a geometrical progression with respect to the time, as above stated. Also it appears that after the lapse of a considerable time of undisturbed cooling, the distribution of temperature in the sphere decreases in proceeding from the centre to the surface, as the quotient of the sine of an arc divided by the arc, the lengths of the arcs being the distances from the centre, and the radius, to which such lengths are made arcs, depending on the conductive and radiative powers.

Besides these solutions of problems respecting the distribution of heat, Fourier has given solutions in other forms, involving definite integrals. By means of such integrals, discontinuous functions may be expressed; and the functions, of which these definite integrals are taken, involve the given function representing the original distribution of the heat. For instance, the problem of the propagation of heat in an infinite line is expressed by an integral of this kind, given by Laplace as the solution of an equation of partial differences*.

By such analytical artifices Fourier solved a number of the problems belonging to this subject; purposely varying his methods, as he says, "à fin de multiplier les moyens de solution dans une matière aussi nouvelle†." Thus, besides the cases already mentioned, of a rectangular solid and a solid sphere, he treats of an "*armille*," or ring, the properties of which are somewhat curious in reference to this subject; also of a solid cylinder, of a rectangular prism, and of a cube‡. A succeeding part of the work contains the laws of the propagation of heat in an infinite solid. This case is of importance, in as much as the conclusions are applied to the propagation of heat in the mass of the earth; which, for such purposes, may be considered as of infinite dimensions. These conclusions deserve to be stated.—When any part of a solid mass is affected by alternations of greater or less temperature, (as the surface of the earth is affected by diurnal and annual alternations,) the following are the results.

1st. The range of the oscillations of temperature becomes smaller as we recede from their origin, and at last they become

* $u = \int d q\, e^{-q^2} \phi(x + 2q\sqrt{kt})$, the solution of $\frac{du}{dt} = k\frac{d^2u}{dx^2}$.

† *Théorie de la Chaleur*, p. 452.

‡ "M. Lamé, professeur de physique à l'Ecole Polytechnique, has determined the law of temperature of all the points of a homogenous ellipsoid brought to a permanent condition. The expression of this law depends on elliptical functions."—Poisson, *Théorie de la Chaleur*, p. 4.

insensible. (Thus the diurnal and annual oscillations of temperature become insensible at a certain depth below the surface of the earth.)

2nd. The oscillations of longest period are sensible to the furthest distance. (Thus the annual alternations are felt at a greater depth than the diurnal.)

3rd. The diminution of the range of the oscillations is less rapid as the conducting power of the substance is greater.

4th. The maximum temperature occurs at different epochs at different distances from the origin. (Thus the maximum of annual temperature is later as we go deeper.)

The skill and resource shown by Fourier in this investigation, and the interesting and instructive nature of the results, make the series of his labours one of the most important portions of the physico-mathematical researches of the present century. His memoirs, as we have said, remained unpublished, except in extracts, till 1824; but they were consulted in the archives of the Institute by MM. Poisson and Cauchy. The former analyst turned his own eminent talents to this subject, and two memoirs of his upon it were read to the Institute, one in May 1815, and one in December 1821; and though not immediately published, were made known by abridgements and extracts in the *Bulletin des Sciences* (May 1815), *Annales de Chimie** (1821), and *Journal de l'Ecole Polytechnique*† (July 1823). In the actual results of the calculation there was no difference between him and Fourier; and he confirmed, for instance, the curious laws which we have just noticed of the propagation of heat in an infinite solid‡. One principal object of M. Poisson appears to have been to establish the fundamental equations by reasonings founded on his own views of molecular action. In these he agreed with Laplace, whose objections to Fourier's reasoning we have already endeavoured to appreciate rightly. M. Poisson, indeed, carries much further than Laplace himself the Laplacian views of molecular action; and has attempted to show the entire insufficiency of Laplace's theory of capillary action, because it does not consider the variation of density which must take place near the surface of a fluid, when it is considered as a collection of discrete particles affecting each other by their mutual attractions. But when it is recollected that M. Poisson obtains for the capillary attraction of a fluid mass the same expression which Laplace obtains; the same constant quantities, borrowed from observation, being involved according to each method, and the difference consisting only in

* *Ann. Chim.*, 19 (1821), p. 337.

† *Journal de l'Ecole Polytechnique*, cah. 19, p. 1.

‡ *Ibid.*, p. 75.

the form of the definite integrals which these constant quantities represent*; we cannot but consider the assertion of the physical falsity of Laplace's view as somewhat arbitrary. Another object of M. Poisson's memoirs on the distribution of heat was to remove some of the mathematical difficulties from which, as he says, Fourier's analysis does not appear to him exempt. These occur in the expression of discontinuous functions, the roots of exponential equations, and similar matters: but such objects do not here form a principal point in the survey we have here to take, and I shall not dwell upon them†.

In the investigations of Fourier and Poisson the functions which we have already spoken of under the name of Laplace's coefficients were not employed. But Laplace himself published in 1820 a memoir on the Cooling of the Earth in which they were made use of‡. Laplace's more general solution includes that of Fourier, who had supposed the temperature to be a function of the distance from the centre only. His conclusions as to the laws of cooling of a sphere agree with those of Fourier's memoir on the secular cooling of the earth, of which an extract appeared in the *Bulletin des Sciences* for 1820 (p. 58.). I will here briefly mention the general consequences which Fourier draws from his solution.

1st. If the globe of the earth had no primitive heat (*chaleur d'origine*), the temperature when we descend below the crust will be constant in each vertical line, and equal to the mean temperature.

2nd. If the heating which is produced by the solar rays have not reached its limit, the temperature will decrease in descending.

3rd. If the temperature increase in descending, there must be some primitive central heat.

4th. If the primitive heat and the solar heat were both dissipated, the temperature of the globe would be that of the planetary spaces.

5th. There is a relation between the excess of the heat of the surface over the heat of the exterior space, and the increase of heat in descending below the surface. Thus an increase of 1 degree in 30 inches descent, supposes that the primitive heat is sufficient to raise the temperature of the surface $\frac{1}{4}$ of a degree above the exterior space, the globe being supposed to have the conducting power of iron.

6th. The primitive heat being supposed to operate, the tem-

* Poisson, *Act. Capill.*, p. 15.

† See Mr. Peacock's Report, pp. 257, 343.

‡ *Conn. des Tems* for 1823, p. 213.

peratures in the interior of the globe are much greater than those of the exterior space. The temperature increases in descending, but the rate of increase becomes slower and slower as we approach the centre.

Before I say anything of the comparison of these and the preceding results with observed facts, I will terminate the history of the subject as a branch of mathematics, or rather as an exemplification of analytical artifices. It is in this point of view mainly that we must consider Count Libri's investigations, read to the Academy of Sciences of Paris in 1825*. He is the only person, so far as I know, who has made the basis of his reasoning Dulong and Petit's exact law, instead of Newton's inaccurate one. He applies his analysis to the case of an armil, in which, as we have already said, Fourier had obtained some curious consequences. We may doubt, however, whether he is justified in reasoning from Fourier's experimental results, so as to modify his own formulæ, (as he does in his memoir, p. 28.)

It is impossible not to look with admiration at the consummate analytical skill with which the mathematicians whose names I have had to mention have explored this subject. At the same time we may be permitted to observe, that the direction which the speculations of our mathematicians concerning heat have thus taken, has not been in all respects favourable to the progress of the subject as a branch of experimental and inductive science. The great beauty and curiosity of many of the mathematical investigations which offered themselves to our analytical discoverers, have led them to wander in that deep and charmed labyrinth much longer and further than the demands of physical science required; and this proceeding has been attended with the additional consequence, that all the cultivators of science, except a very few, well equipped for the mathematical race, have been left behind by the course of discovery, and have almost lost sight of their leaders. There can be no doubt that this might have been otherwise;—that the subject might have been treated by means of mathematics of a simpler kind; such, for instance, as Newton would have employed, had his steps turned into this train of inquiry. This would in all probability have been attended by some sacrifice of rigour and of generality, and the highest analysis would always have been requisite, in order to obtain the best solution, as we see in the problem of vibrating cords, with which the problems of the distribution of heat have many points of resemblance. But still such solutions would have been just in all the material points; and, by showing to common students the nature of the operations

* *Mém. de Math. et de Phys.* Florence, 1829.

and relations in question, and the possibility of tracing them mathematically, they would have brought a far wider circle of intellect to bear upon the inquiry; and thus would have tended much to the diffusion of sound knowledge, and, not improbably, to the promotion of further discovery. Even now, any mathematician who would present the subject to students in such a generally accessible form, would probably find himself rewarded both by the simplicity and elegance of the propositions to which he would be led, and by the utility which his labours would be found to possess.

4. *The Application of the theory of heat to questions* which occur as important subjects of speculation in natural philosophy, is the last division of our task. We may notice as belonging to this division three investigations: The effect of the Solar Heat upon the Earth, and the laws of its Distribution; the effect of the Primitive Central Heat, and the evidence of its existence; the effect of the Proper Heat of the Planetary Spaces, and the evidence that such heat exists.

(*a.*) With regard to the first point, I have already stated the conclusions respecting the propagation of alternations of heat and cold in a solid; and I have now only to add, that several observers have ascertained by experiment that the propagation of the solar heat into the interior of the earth does follow the rules which theory points out; namely, that the range of oscillation of temperature becomes narrower and narrower as we recede from the surface; that the annual oscillations are sensible to a much greater depth than the diurnal; and that the maximum heat and maximum cold occur later below than at the surface, and later and later the deeper we descend. These rules, in the whole or in part, were traced by Ott at Zurich in 1762, by Saussure in 1785, by Hemmschneider at Strasburg in 1821 and the two following years, but most completely by Leslie in 1816 and 1817*. These observers and others have found that at a certain depth, as 40, 50, or 60 feet, the temperature ceases to show the effect of the changes of solar influence; we have an *invariable stratum*: at smaller depths the rules are such as have been stated. It is, moreover, to be observed, that the temperature of the invariable stratum is different in different places; and if we assume, as an approximation to the actual condition of the earth, that the equator is kept at a constant high temperature, we shall have a constant flow of heat in the interior of the sphere from the equatorial to the polar

* Pouillet, *Elémens de Météorologie*, tom. ii. p. 643; where Leslie's experiments, made in the grounds of Mr. Ferguson of Raith, are erroneously attributed to that gentleman.

regions. In the actual condition of the earth, acted on as it is by the sun, we have alternations in the influx and efflux of heat, but on the whole the same result as by the above theoretical view; an influx in the equator parts, an efflux at the poles, a circulation in the interior, and a diminution of the mean temperature in proceeding towards either pole *. The formulæ which represent the empirical laws of the dependence of the mean temperature on the latitude, have been stated by Professor Forbes in his Report on Meteorology (p. 215); namely, the old one of Mayer, which made the temperature nearly proportional to the square of the cosine of the latitude; and other more exact rules since proposed by Brewster and others. I am not aware that any attempt has been made to bring these rules into accordance or even comparison with Fourier's theoretical formulæ. Such an accordance would be extremely interesting in its bearing on the theory.

It is curious to obtain from the theory the actual amount of the solar heat annually poured upon the earth. This is stated† to be sufficient to melt a coat of ice 14 metres thick, encrusting the whole globe of the earth.

(*b.*) The subject of Central Heat is of great interest to the geologist; and, as we have already seen, is illustrated by the theory. It appears, that if there be an increase of temperature in descending, such a fact can result from nothing but a central heat independent of existing influences. The discussion of the evidence of this fact must be left to the geological speculator; but we may here mention some of the results of theory which are fitted to make less formidable the idea of having a vast abyss of incandescent matter within the comparatively thin crust of earth on which man and his works are supported. It results from Fourier's analysis ‡, that at 20,000 or 30,000 metres deep the earth may be actually incandescent, and yet that the effect of this fervid mass upon the temperature at the surface may be a scarcely perceptible fraction of a degree. The slowness with which any heating or cooling effect would take place through a solid crust is much greater than might be supposed. If the earth below 12 leagues' depth were replaced by a globe of a temperature 500 times greater than that of boiling water, 200,000 years would be required to increase the temperature of the surface by 1 degree§. A much smaller depth would make the ef-

* Fourier's formula is $v = \cos. x \int e^{y \cos. r}\, dr$, where cos. $x$ is the sine and $y$ the cosine of the latitude, and the integral is taken from $r = o$ to $r = \pi$. (Fourier, *Mém. Inst.*, tom. v. p. 173.)

† Pouillet, tom. ii. p. 704.

‡ *Bullet. des Sci.* 1820, p. 58.

§ *Mém. Inst.* vii. p. 603.

feet on the superficial temperature insensible for 2000 years. It is calculated, moreover, that, from the rate of increase of temperature in descending, the quantity of central heat which escapes in a century through a square metre of the earth's surface would melt a column of ice having this metre for its base, and three metres for its height*.

(*c.*) *The remaining subject, the Heat of the Planetary Spaces, is one on which the scientific world has hardly yet had time to form a sage and stable opinion.* Fourier has asserted† that, without the effect of this heat, the diminution of temperature in proceeding from the equator to the pole would be much greater than it is found to be; that the variations of distance of the sun at different parts of the year would be felt in changes of temperature; that the alternate heat and cold of days and nights would produce oscillations of temperature more violent than those which occur. He infers that there exists a cause which moderates the temperatures at the surface of the globe, and which produces a fundamental temperature independent of the sun and of the central heat. This cause he holds to be the heat of the planetary spaces, and he ascribes this heat to the radiation of the fixed stars in every part of the universe. Fourier was led by his reasoning to fix the temperature of the planetary spaces at about 50 degrees (centigrade) below freezing. It is curious that Svanberg has been led by a wholly different line of reasoning to nearly the same result as to the degree of temperature of the void spaces of our system.

Fourier says that his conclusion results from the mathematical examination of the question; but the mathematical part of his reasoning on this subject has not, so far as I am aware, been published. Sir John Herschel has, I believe, dissented from the opinion that such an effect could result from the radiation of the fixed stars; but his memoir also is as yet unpublished.

Thus there appears to be still some uncertainty on the subject of what we may call cosmical heat. Indeed the extension of our thermotical laws to cosmical cases appears to be attended with great difficulty. The laws of motion were first strictly proved by experiments made with bodies capable of manipulation; but they were immediately applied to the explanation of cosmical phenomena of which the laws had long been ascertained by observation. It should be the aim of other sciences to make similar applications of their results. But in most cases the difficulty of observation of phenomena at a distance from us is very great. We can observe the effects of mechanical force in the remotest regions of the universe into which

* *Mém. Inst.*, tom. vii. p. 593. † p. 580.

our telescopes can penetrate; but how little can we learn about the effects of heat or chemistry, of electricity and magnetism, in any substances except those which we can handle! In the case of heat, we can hardly catch any indications of its amount either above or below a thin crust at the earth's surface to which we are confined.

Yet in these cases theory is especially to be cultivated, because its calculations are the only instruments by which we can reach into other parts of our system;—by which we can pass the bounds of space and time which at first sight appear allotted us. Something has been done in this way: the magnetic changes which the globe of the earth undergoes have long been studied, and will now be studied still more; the characters in which the electricity of the upper regions of the atmosphere is written, may, perhaps, soon be more clearly interpreted than they yet have been; and, with regard to heat, Fourier has shown, that if we had ancient observations of the rate of increase of temperature in descending, to compare with those recently made, we should be able to infer the actual temperature of points at a distance below the surface, and the former temperature of the surface.

All these prospects afford reasons both for further cultivating the theories of these subjects and for making accurately those observations which the theories point out as important elements of calculation. In the course of this Report some tasks of both kinds have been indicated as more peculiarly desirable; and I will conclude by again stating them as briefly as possible. They are such as follow: A comparison of good recent measures in electrical experiments (those of Mr. Snow Harris and any others) with the Coulombian theory; a determination of the degree of exactness of compensation attained and attainable by means of Mr. Barlow's correcting plate; the measure of the rate of increase of temperature of the earth's mass in descending (both in given places and on the average), to compare with similar observations at a future period; the comparison of the observed law of temperatures, as depending on the latitude, with Fourier's formulæ; and, finally, as a humble but most useful step, the production of treatises in which the results of the theories above spoken of, (Coulomb's theories of electricity and magnetism, and Fourier's theory of heat,) shall be presented in a manner sufficiently elementary to be accessible to mathematicians of common attainments, as, for instance, to the readers of Newton.

*Note.*—The statement made in p. 31, that an increase of temperature in descending can result from nothing but a central

heat, is contested by M. Poisson in his *Théorie de la Chaleur*, published since this Report was written. For M. Poisson's view of this subject, see the Report of the Proceedings of the Geological Section, (p. 489 of the *Lond. and Edinb. Phil. Mag.* for Dec. 1835.)

## INDEX.

*Aperçu de l'Etat actuel des Sciences Mathématiques chez les Belges.* *Par* A. Quetelet.

L'histoire intellectuelle d'un peuple se rattache par tant de liens à son histoire politique, qu'on ne peut guères séparer l'une de l'autre. Jusqu'à présent les écrivains qui se sont occupés de l'histoire des sciences, se sont plus attachés à faire connaître les résultats qu'elles ont produits, que les causes sous l'influence desquelles elles se sont développées, et qui ont pu, à différentes époques, en favoriser ou comprimer l'essor.

Cependant la recherche des causes qui influent sur l'état des lumières est éminemment philosophique, surtout lorsque cette recherche se fait dans des vues spéciales, pour améliorer l'état d'un peuple et lui imprimer une impulsion utile.

On aurait également tort, quand on ne veut pas s'en tenir à la surface des choses, de considérer dans l'histoire des sciences une époque en dehors de tout ce qui l'a précédé. On pourrait connaître ainsi l'état intellectuel de cette époque, mais on ne saurait nullement s'il est le résultat d'un progrès ou d'une décroissance de lumières.

C'est par ces motifs que voulant donner un aperçu de l'état actuel des sciences mathématiques et physiques chez les Belges, j'ai cru qu'il ne serait pas hors de propos de jeter un coup d'œil rapide sur ce que les sciences ont été antérieurement, et sur la disposition des esprits à les étudier. On concevra mieux ensuite ce que les savans peuvent encore attendre de ce côté.

Lorsqu'une branche des connaissances humaines est acclimatée dans un pays, quand les masses en ont senti la salutaire influence, et qu'on y trouve de l'honneur ou du profit à s'y distinguer, on ne doit plus désespérer de son avenir. Les hommes éminens s'y développent spontanément, et y atteignent la plus heureuse maturité, comme les fruits dans un terrain convenablement préparé.

En général, les sciences et les lettres, de même que les beaux-arts, s'établissent de préférence chez les peuples riches, et sous l'influence de gouvernemens protecteurs. Elles ne pouvaient donc manquer, dès la renaissance, de fleurir en Belgique de l'éclat le plus brillant. La pompe fastueuse de la Cour de Bourgogne, la magnificence de ses Ducs, et l'état prospère de la nation, furent également favorables au développement de toutes les branches de l'intelligence humaine. La création récente de

l'Université de Louvain fut, d'une autre part, un stimulant actif, surtout pour la propagation des études solides; aussi l'on vit s'élever à côté des Froissart, des Commines, des Monstrelet, des Chastelain, et des Molinet, ces historiens dont les écrits ont donné tant de relief à la Maison de Bourgogne, les Despautère, les Clenard, les Viglius, les Premacle de Florenne et tant d'autres écrivains dont les ouvrages servirent de base aux études solides. Il appartenait aussi à cette brillante époque de donner naissance à la peinture à l'huile, et aux chefs-d'œuvre des Van Eyck et des Hemmelinck. La musique, dont l'art était à peu près perdu, se ranima par les travaux de Guillaume Dufay, de Jean Okeghem, de J. Teinturier et d'une foule d'artistes savans qui se répandirent par toute l'Europe, et qui ne sont pas encore oubliés même dans les pays les plus renommés pour l'art musical. La poësie ne fut pas négligée dans ce mouvement général; et les écrits de Van Maerlant surtout peuvent en servir de preuve. Les sciences comptaient également des hommes distingués pour cette époque; déjà même pendant le 13e et le 14e siècle, Ægidius de Lessine, Henri Baten, Henri de Bruxelles, et Henri de Gand, proclamé de son temps le *Doctor Solemnis*, se distinguaient dans les sciences physiques.

Cette puissante impulsion donnée aux lettres, aux sciences et aux beaux-arts par la Maison de Bourgogne, les avait en quelque sorte acclimatés: le plus difficile était fait; l'opinion publique s'était déclarée en leur faveur, et chacun savait qu'il y avait de l'avantage à s'y distinguer. Les hommes les plus éminens avaient accès auprès de leurs princes, et plusieurs même étaient reçus dans leur intimité. Pendant son règne éclatant, Charles V., ce puissant rival d'un des princes qui ont le plus protégé les lumières, continua l'ouvrage des Ducs ses prédécesseurs; les hommes les plus distingués de cette époque furent appelés à sa cour; et si plus tard, sous le règne de son fils, la main fatale du Duc d'Albe s'appesantit sur la malheureuse Belgique, la crise ne se prolongea pas assez longtemps pour que le Gouvernement protecteur d'Albert et d'Isabelle ne pût encore en réparer les maux. Le seizième siècle ne fut donc point inférieur à celui qui l'avait précédé; mais comme la présence du prince ne venait plus vivifier les sciences, et que l'action gouvernementale s'imprimait par des intermédiaires, la Belgique continuait à produire des hommes distingués, mais à mesure qu'ils se développaient, ils allaient porter leurs talens à l'étranger, soit par le désir d'acquérir des biens et des honneurs, soit par le besoin d'échapper au pouvoir ombrageux et despotique du gouvernement de Philippe II. Le règne glorieux d'Albert et d'Isabelle, qui termina pour nous d'une manière si heureuse le seizième

siècle, ne put entièrement arrêter cette émigration, qui était pour ainsi dire un besoin, et qui devint à peu près générale, quand le traité de Munster, plus tard, ferma les bouches de l'Escaut, et porta à la Belgique l'un des coups le plus rudes qu'elle ait jamais éprouvés.

A partir de cette époque, et surtout après le fatal traité des barrières, la prospérité du commerce déclina, et avec elle tout ce qui distingue le plus un peuple. On vit successivement s'éteindre le goût de la musique et de la poësie; les sciences et les lettres eurent leur tour; et la peinture même, dont l'avenir semblait le plus assuré, la peinture qui doit à jamais immortaliser le nom des provinces Flamandes, ne put échapper entièrement au malheur qui désola notre pays.

Pour faire apprécier le mal, il suffira de rapporter, d'après un de nos historiens, quelles furent les suites de ce fameux traité des barrières (1715): "Il n'y a pas d'exagération à dire qu'il fut avec l'article du traité de Munster sur la navigation de l'Escaut, l'œuvre qui consomma la ruine des Pays-Bas. Prise isolément, cette convention n'avait pour objet que de poser un frein à l'ambition de la France. Dans ce sens, elle était dans nos intérêts comme dans ceux des Provinces Unies; mais on doit la regarder comme une dépendance du traité d'Utrecht, et sous ce point de vue finances, commerce, industrie, liberté, indépendance, tout ce que les hommes ont de plus cher y fut compromis : nos places les plus importantes furent occupées par les troupes étrangères; c'était avec nos fonds qu'on les soudoyait. Toutes les entraves que des rivaux d'industrie peuvent imaginer furent imposées à notre commerce, nos ports fermés aux vaisseaux étrangers, les routes maritimes interdites à nos marins; liés par des lois fiscales étrangères, à la merci d'un système intérieur de douanes ouvrage de nos adversaires, nous ne pouvions faire un pas dans la route des innovations sans rencontrer des obstacles; rendre une loi salutaire, élever une institution bienfaisante ou une compagnie d'industrie, sans exciter les cris de nos voisins et nous attirer les menaces de l'Europe entière*."

Au milieu de tant de désastres, les beaux-arts, les sciences et les lettres perdirent successivement l'éclat dont ils avaient brillé: qu'on ajoute à cela que les gouvernemens qui nous arrivaient de l'étranger ne connaissaient ni nos goûts ni nos besoins, et s'inquiétaient fort peu de la gloire nationale. Trop heureux encore si les hommes qui se distinguaient parmi nous n'avaient pas à souffrir des humiliations. On rapporte que l'un d'eux†, Lan-

* Tome vii. des *Mémoires Couronnés de l'Académie Royale de Bruxelles*, émoire de M. Steur, page 40.

† Foppens, *Bibl. Belgica*, p. 691.

grenus, ou plutôt Van Langren, cosmographe instruit et dont le nom est resté dans les sciences, ayant composé sa Sélénographie vers 1674, en présenta les dessins à l'Archiduc Leopold, Gouverneur des Pays-Bas. Ce prince, pour toute recompense, lui dit en ricanant: "Je vous nomme gouverneur des terres que vous avez découvertes dans la Lune." Le pauvre astronome se contenta de lui répondre: "Je remercie Votre Altesse, si elle veut bien me faire donner les provisions nécessaires pour le voyage."

Le dix-huitième siècle, qui suivit ces temps désastreux, ne nous présente plus d'hommes marquans dans les sciences; nous n'avions plus le gouvernement Espagnol, ni son faste, ni les richesses qu'il versait dans notre pays; ce n'étaient plus ces luttes, ces agitations qui donnent quelquefois du ressort aux esprits.

Le gouvernement Autrichien s'était, à son tour, chargé du soin de notre avenir. Un sommeil profond, un sommeil semblable à celui de la mort, s'était répandu dans toutes nos provinces. Ce n'est pas que le peuple, sous un rapport, fut essentiellement malheureux; d'ailleurs comment aurait-il pu apprécier le bien dont on l'avait deshérité? il en était venu à cet état où l'on ne peut plus, où l'on n'est plus stimulé par le désir de la gloire, où l'on finit par oublier ses titres les plus nobles, uniquement occupé de satisfaire aux besoins matériels de la vie. L'Université de Louvain, qui seule aurait pu ranimer les esprits assoupis, partageait elle-même l'état d'engourdissement général; elle ne se tenait plus au courant des découvertes qui illustraient le siècle, et jouissait des débris de son ancienne renommée. La révolution que venaient de produire, dans l'analyse mathématique, les travaux de Newton et de Leibnitz, parcourut l'Europe entière, mais elle ne laissa point de traces en Belgique; et sans les ouvrages du Commandeur De Nieuport, on pouvait se demander, il y a bien peu de temps encore, si quelqu'un parmi nous s'était occupé du calcul infinitésimal, depuis l'époque de sa naissance. L'état d'abandon des sciences mathématiques devait nécessairement influer sur toutes les sciences qui en dépendent. De là, ce vide affreux dans nos annales, cette absence complète d'observations de toute espèce, soit pour la météorologie de notre pays, soit pour le magnétisme terrestre, soit enfin pour tout ce qui tient à la physique et à l'astronomie. Aussi, qu'on ne nous demande pas ce qui s'est passé chez nous, pendant un siècle entier; notre planète aurait pu échapper à son orbite, que nous n'en aurions rien su, tant notre sommeil était profond! étrange état de marasme qui succéda à des siècles où non seulement nos Belges marchaient, dans les sciences et les

arts, les rivaux des autres peuples, mais où même ils étaient, pour ainsi dire, en possession de leur donner des maîtres.

Une femme chercha à nous tirer de cet état de léthargie, et elle s'est acquis à jamais des titres à notre reconnaissance. Marie Thérèse, de glorieuse mémoire, fut secondée dans ses desseins par le Comte de Cobentzl, son ministre plénipotentiaire au gouvernement des Pays-Bas. Ce ministre, qui était éclairé et qui savait honorer les sciences, cherchait à faire renaître l'ancienne splendeur dont elles avaient brillé en Belgique. Il était choqué de voir combien peu l'Université de Louvain répondait au but de son institution: "Il est honteux," disait-il, "que nous ayons dans notre Université des gens si peu faits pour maintenir le bon goût, et entièrement livrés à la barbarie pour les sciences et à la rusticité pour les mœurs*." Pour remédier à ce mal, il proposa à l'Impératrice la création d'une société littéraire à Bruxelles, qui trois ans après, en 1772, fut érigée en Académie Royale et Impériale des Sciences et Belles Lettres. Il est remarquable que dans la classe des sciences les membres les plus distingués furent presque tous des étrangers: tant un siècle d'intervalle avait changé l'état des choses.

L'Académie, à sa création, se trouvait assez embarrassée pour expliquer, sans blesser son auguste fondatrice, l'état de torpeur dont elle cherchait à faire sortir le pays. C'est ce qu'on peut voir par le discours d'introduction à ses mémoires, où, après avoir fait l'énumération de la prospérité matérielle du pays, elle ajoute: "Les lettres furent négligées, soit que l'attention de guérir les plaies de l'état occupât seule le soin du Gouvernement, soit par d'autres causes qu'il serait inutile d'approfondir; elles demeuraient dans un état de langueur qui empirait de jour en jour." Les preuves ne nous manqueraient pas pour montrer combien le mal était devenu grand, et avait pénétré même dans les corps les plus élevés. Dès sa naissance l'Académie avait proposé des questions qui annonçaient des vues étendues et philosophiques. Elle avait appelé l'attention sur l'ancienne organisation politique du pays, quelques membres des Etats firent des démarches pour empêcher que de semblables sujets fussent publiquement débattus; l'Académie eut le courage de mépriser leurs menaces, et le Gouvernement le bon esprit de ne pas les sanctionner†.

Ce corps ne se montra pas indigne de sa mission: il publia, pendant sa courte existence, cinq volumes de mémoires de ses

* Voyez la notice biographique de ce savant par M. De Reiffenberg, dans l'*Annuaire de l'Académie*, page 85.

† M. De Reiffenberg, *Ann. de l'Académie* 1835, p. 86.

membres, et un grand nombre de mémoires couronnés sur différentes branches des connaissances humaines. Néanmoins les sciences physiques et mathématiques furent peu cultivées : pour les sciences mathématiques, le Commandeur de Nieuport fut le seul qui s'en occupa dans son sein, et l'on peut dire dans nos provinces; les sciences physiques furent représentées par des savans étrangers, MM. Pigott, l'Abbé Needham et l'Abbé Mann, tous trois Anglais, mais qui s'étaient établis parmi nous. Du reste, les choses étaient loin d'être organisées sur un pied convenable; il n'existait encore aucune ressource pour cultiver les sciences d'observation; l'Académie s'en plaignait; et quand elle fut invitée par la Société Palatine à prendre part au grand système d'observations météorologiques combinées qui s'organisait alors, elle exprima la crainte d'entreprendre ces observations, ou d'autres travaux de la même nature, parcequ'il lui manquait des instrumens et un observatoire*. La Société Palatine lui envoya donc ce qui était nécessaire, et les observations demandées furent faites avec régularité. Il en fut de même, quand M. Pigott, gentilhomme Anglais, se fixa parmi nous pour coopérer à un grand travail désiré par le Gouvernement, et qui consistait à rectifier la carte du pays; non seulement il fut forcé de faire venir des instrumens d'Angleterre, mais il fit ce travail *gratuitement et même à ses frais*, comme le rapporte Lalande dans le 5<sup>e</sup> volume de l'*Histoire des Mathématiques* de Montucla, p. 353.

Cependant on remarquait des améliorations sensibles, quand arriva la grand catastrophe qui termina le 19<sup>e</sup> siècle, et qui arracha la Belgique à l'Autriche pour la jeter dans les bras de la France.

L'Académie avait été supprimée et ses membres dispersés; l'ancienne Université de Louvain, dont l'agonie avait été si longue, n'existait plus; la plupart des ouvrages précieux de nos bibliothèques, et les chefs-d'œuvre de l'école Flamande avaient été transportés à Paris, pour alimenter ce vaste foyer qui éclaire le monde, et dont la France paie généreusement les frais. Dans cet état de choses la Belgique s'effaça de nouveau. Cependant les sciences avaient pris en France un essor trop élevé, elles jetaient un éclat trop vif pour qu'il n'en rejaillit pas des étincelles jusqu'au fond de nos départemens. Les écoles centrales d'abord, et les lycées ensuite, répandirent parmi nos jeunes gens

* "Attamen haud silendum arbitramur, nos in præsenti rerum statu quodammodo vereri, ut cujuscunque generis observationes à nobis fieri possint; deest enim hucusque locus ad observandum aptus, speculatoria turris undè motus siderum investigaretur, deest et multa supellex ad res meteorologicas requisita."—*Ephemerides Soc. Meteor. Palatinæ*, ann. 1781.

le goût des sciences exactes, qu'ils pouvaient aller cultiver dans l'école la plus célèbre des temps modernes, et sous les yeux des hommes les plus distingués de l'époque. On savait que les sciences étaient honorées, que jamais leur puissance n'avait été plus grande; on savait que l'homme dont la gloire militaire retentissait alors par toute l'Europe prenait à cœur de répandre sur elles un partie du prestige qui l'environnait, et qu'il avait élevé les savans les plus illustres à la dignité de princes et de premiers fonctionnaires de l'empire. Cette munificence, qui honorait bien plus celui qui en usait que les savans qui en étaient l'objet, entretint cette source d'illustrations qui avait pris naissance au milieu de l'exaltation révolutionnaire.

Cependant la guerre qui parcourait successivement les différens pays de l'Europe, et les grands travaux qui s'exécutaient dans l'intérieur de l'Empire, absorbaient trop nos jeunes Belges sortis de l'Ecole Polytechnique pour leur permettre de se livrer aux paisibles travaux du cabinet; et quand, plus tard, la paix les rendit à leur patrie, la plupart avaient perdu depuis trop longtemps de vue les spéculations scientifiques pour pouvoir s'y remettre encore avec succès.

Lorsque la France ouvrit son Institut, la Belgique n'y fut représentée que par deux de ses savans; et pendant tout le temps de sa réunion à la France, elle n'en eut point d'autres: c'étaient le Commandeur De Nieuport et M. le Professeur Van Mons. Le premier de ces savans, que la tourmente révolutionnaire avait dépouillé de tous ses biens, cultivait avec fierté dans sa retraite les sciences mathématiques, qui l'avaient autrefois mis en rapport avec Condorcet et D'Alembert; et l'état d'isolement auquel il s'était condamné ne lui permit pas d'exercer une grande influence sur ses concitoyens. M. Van Mons, au contraire, doué d'une activité incroyable, en possession de la plupart des langues de l'Europe, et en relation avec les hommes les plus distingués de l'époque, s'était rendu pour ainsi dire l'intermédiaire entre le nord et le midi; il transmettait à l'Angleterre et à l'Allemagne les brillantes découvertes de Volta et de Lavoisier, dont il défendait avec ardeur les théories nouvelles, tandis qu'il faisait connaître en France les découvertes des savans du nord*.

Tel était l'état de la Belgique quand les événemens de 1814

* M. Van Mons publia aussi de concert avec MM. Bory de St. Vincent et Drapiez en 1819, les *Annales Générales des Sciences Physiques*, dont le huitième et dernier volume a paru en 1821. Cet intéressant recueil était surtout consacré aux sciences naturelles. Les mathématiques n'y étaient pas représentées; et à l'exception d'un mémoire sur les expériences de M. Nelis sur la perméabilité du verre au fluide électrique, on n'y trouve guères de documens pour l'histoire de la physique dans nos provinces.

la détachèrent encore de la France et lièrent ses destins à ceux de sa rédoutable rivale, à l'héritière de tous les bénéfices du traité de Munster et de celui des barrières qui avaient été pour notre pays une source de calamités. Cependant l'union n'avait pas lieu pour la Belgique, à titre de dépendance, mais bien d'égalité; de sorte que ses représentans se crurent en droit de réclamer pour elle les mêmes avantages et les mêmes institutions libérales dont les Provinces-Unies avaient continué à jouir depuis l'époque de nos désastres. Ces provinces avaient conservé ce même amour et ce même respect pour les sciences qui s'étaient successivement éteints chez nous; elles enrégistraient les titres de leurs savans à côté de ceux de leurs hommes de guerre, et elles les citaient avec orgueil aux étrangers; c'étaient là leurs titres de noblesse.

Cet amour des sciences dont la Hollande avait conservé les traditions fut pour nous d'un immense avantage: nous nous trouvâmes en droit de réclamer pour nos provinces les mêmes bienfaits dont elle jouissait depuis longtemps; et l'on doit convenir, que le Gouvernement ne recula pas devant des demandes aussi légitimes. Peu de temps après la réunion des deux pays, nous eûmes trois universités, comme les provinces du nord; l'Académie de Bruxelles fut rouverte aux sciences et aux lettres; on créa des musées, des jardins botaniques; on augmenta les bibliothèques, et l'on vit se former un observatoire, monument que nous n'avions jamais possédé jusqu'alors, et qui même était conçu sur une échelle plus grand que tous les autres observatoires des provinces du nord.

Tant d'établissemens nouveaux exigeaient un nombreux personnel; et quoique la Belgique commençât à compter un assez grand nombre d'hommes distingués, il se trouvait encore beaucoup de lacunes dans différentes branches d'enseignement. Le Gouvernement appela donc des savans étrangers, auxquels il réunit d'autres savans venus des provinces septentrionales. Notre orgueil national, trop susceptible, vit avec peine ces différens appels; et l'on doit convenir que plusieurs professeurs étrangers ne tinrent peut-être pas assez compte des circonstances facheuses dans lesquelles ce pays s'était trouvé, et qu'ils furent loin de respecter ces susceptibilités nationales. Tous les choix d'ailleurs n'avaient pas été également heureux. De là, en grande partie, la défaveur qui s'attacha aux Universités naissantes, malgré les services réels qu'elles rendirent.

La *réorganisation* de l'Académie de Bruxelles ne fut pas non plus favorablement accueillie. On parut oublier entièrement les services rendus par ce corps savant dont peu de personnes chez nous connaissaient les travaux; on voyait d'ailleurs dans l'Aca-

démie nouvelle la plupart des membres du corps enseignant. L'Académie d'une autre part ne cherchait pas à vaincre ces préjugés: satisfaite en effet des témoignages d'estime qu'elle reçevait des étrangers et du peu de savans qui chez nous se tenaient au courant de ses publications, elle travaillait dans le silence et semblait éviter les occasions de se mettre en contact avec le public, qui de son côté ne vit dans cet isolement qu'un esprit de dedain et qu'une espèce d'aristocratie scientifique. L'Académie ne se rebuta point, et, avec la plus louable constance, elle jeta les bases de grands travaux qui, plus tard, lui vaudront sans aucun doute la reconnaissance de la nation.

Il s'éleva donc contre les Universités, l'Académie, et les grands établissemens scientifiques, d'assez fortes préventions qui furent préjudiciables au progrès des sciences; et c'est devant ces préventions que faillirent s'abimer toutes ces grandes institutions quand éclata la révolution de 1830. Ces souvenirs sont d'autant plus douloureux pour l'homme de science qu'ils sont plus récens; mais l'historien ne peut les taire, quelqu'affligeant que le récit en soit pour son patriotisme. Des mains maladroites portèrent d'abord la hache dans les universités de l'état, et les coups furent tels, qu'elles n'ont pu se rétablir depuis, et qu'une réorganisation complète devient de plus en plus urgente; quelques voix demandaient la suppression de l'Académie, et le refus de subsides pour nos grands établissemens, dont plusieurs même n'étaient pas entièrement achevés; ainsi l'on proposa de convertir le naissant observatoire en abattoir, en hôpital des cholériques, ou en magasin à poudre. Mais, hâtons-nous de le dire, le bon sens repoussa ces folles exigences comme indignes de la nation. On comprit combien il y aurait eu de honte à profiter des premiers instans de notre émancipation politique pour ruiner tous les monumens scientifiques dus à un gouvernement que l'on peignait comme oppresseur de la pensée. Mais pourquoi rappeler ces souvenirs, quand le danger a cessé d'exister, et que chaque jour on comprend mieux combien un peuple ajoute à sa dignité, en donnant des asyles aux sciences et de l'appui à ceux qui les cultivent.

Je viens de tracer rapidement les différentes phases que les sciences ont présentées en Belgique; je vais tacher d'énumérer maintenant les principaux travaux qui ont été produits dans ces derniers temps; s'ils sont moins nombreux et moins importans que ceux qu'ont fait naître des pays plus favorisés, on doit surtout en attribuer la cause aux circonstances dans lesquelles s'est trouvée la Belgique.

*Mathématiques.*—Le Commandeur De Nieuport, comme déjà nous l'avons dit, a été, pendant sa longue carrière, pour ainsi

dire l'unique représentant des sciences exactes en Belgique. Ses premiers travaux furent réunis dans un recueil dont un volume parut en 1794, et un second en 1799, sous le titre de *Mélanges Mathématiques* *; ils sont surtout relatifs à l'intégration des équations aux différentielles partielles. Les *Mémoires de l'Institut* de France renferment aussi un de ses écrits *sur l'équation générale des polygones réguliers*, et un autre *sur un problème présenté par D'Alembert*. En 1802 il donna une suite à ses mélanges, et publia des recherches sur l'Intégrabilité médiate des équations différentielles d'un ordre quelconque et entre un nombre quelconque de variables. Par intégration *médiate*, l'auteur entend l'aptitude à devenir une différentielle exacte au moyen d'un facteur. Les pertes nombreuses que M. De Nieuport avait faites pendant la révolution, l'état d'isolement dans lequel il vivait, et le manque absolu de savans avec qui il pût causer de sa science de prédilection, avaient donné un autre cours à ses idées; il s'était tourné vers la philosophie et la littérature ancienne, qui lui donnaient de douces consolations dans ses revers. Il paraissait avoir perdu entièrement de vue les recherches mathématiques, quand la réorganisation de l'Académie de Bruxelles, dont il avait été l'un des anciens membres, vint le rendre à ses premiers travaux. Il inséra, dans les recueils de ce corps savant, différens mémoires dans lesquels on retrouve des idées ingénieuses, mais qui avaient moins pour objet de faire avancer la science que de perfectionner quelques détails. Les uns concernent la métaphysique du calcul différentiel, ou présentent des réflexions sur les notions fondamentales en géométrie; d'autres se rapportent à des problèmes du calcul des probabilités, ou à des propriétés des lignes du second ordre. Les principaux travaux mathématiques, composés pendant la vieillesse du Commandeur De Nieuport, sont les suivans:

Esquisse d'une méthode inverse des formules intégrales définies.

Sur l'équilibre des corps qui se balancent librement sur un fil flexible, et sur celui des corps flottans.

Sur la pression qu'exerce un corps sur plusieurs appuis à la fois.

Cette dernière question avait été traitée déjà par Euler dans un mémoire ayant pour titre: *De pressione ponderis in planum cui incumbit*, inséré dans les *Recueils de St. Petersbourg*. Cet illustre géomètre y donne les formules relatives au calcul des pressions exercées par un corps pesant sur les appuis du plan inflexible qui le soutient. Son hypothèse, comme on sait, con-

* In 4to, à Bruxelles, chez Lemaire.

siste à exprimer la pression sur chaque appui par l'ordonnée correspondante d'un même plan. Les conditions de l'équilibre du système suffisent alors pour déterminer les constantes arbitraires qui entrent dans l'équation générale du plan.

D'Alembert s'était occupé du même problême dans le tome viii. de ses *Opuscules*; et il pensait que l'indétermination apparente de la question dépendait d'un principe encore inconnu en mécanique, ou de l'emploi d'un principe connu auquel on n'avait pas songé.

M. De Nieuport, après avoir examiné le travail d'Euler, finit par conclure que la solution de la question devait dépendre du minimum ou du maximum de quelques fonctions des divers élémens qui constituent les données du problême, et il offrit plusieurs considérations à l'appui de cet aperçu.

M. Pagani reprit la même question en 1823, et présenta à l'Académie de Bruxelles un mémoire dans lequel, en supposant, comme tous les géomètres qui s'étaient occupés de ce point de statique, que la forme du système était invariable, il établissait *à priori* que *la somme des carrés des pressions doit être un minimum*; il fit voir ensuite que ce principe conduisait à l'hypothèse d'Euler.

M. A. Timmermans, de son côté, présenta plus tard à l'Académie un mémoire *sur les pressions et torsions*, dans lequel il montra qu'on arrivait aussi à l'hypothèse d'Euler, en admettant, comme point de départ, que le polygone formé par les points d'appui est décomposé dans tous les triangles possibles, et que le poids peut être considéré comme supporté par chacun des triangles qui passent sous lui. La charge de chacun de ces triangles est le poids divisé par le nombre des triangles. Quant à la position du point d'application dans chaque triangle, elle est connue; on conçoit donc la possibilité d'exprimer analytiquement la pression exercée sur chaque point.

Les deux mémoires dont il vient d'être parlé ne sont point encore publiés; cependant M. Pagani a inséré dans le tome viii. (1834) des *nouveaux Mémoires de l'Académie de Bruxelles* une note, dans laquelle il résume son travail, et il revient sur la même question, en ayant égard cette fois à la déformation du systême, ce qui fait disparaître l'indétermination qui existe effectivement dans le cas général où la forme du systême est supposée invariable.

Nous devons ajouter que le problême d'Euler concernant les appuis a été aussi traité dans ces derniers temps par M. Fourier sous un point de vue particulier, au moyen de son ingénieuse théorie du *calcul des inégalités*, et par M. Navier dans le *Nouveau Bulletin de la Société Philomatique pour* 1825.

M. Garnier, à qui l'on doit un grand nombre d'ouvrages élémentaires justement estimés sur les différentes branches des mathématiques, avait été appelé comme Professeur à l'Université de Gand dès la première organisation de cet établissement. Ses connaissances étendues et ses relations avec la plupart des mathématiciens les plus distingués de la France, l'avaient mis à même de donner une impulsion favorable à l'étude des sciences exactes. Il a publié successivement des éditions nouvelles de la plupart de ses ouvrages, et il y a introduit des améliorations utiles. Il a aussi fait paraître un mémoire *sur les machines* *, dans lequel il s'est attaché à fondre et à coordonner en corps de doctrine les matériaux épars sur cette partie intéressante de la mécanique, en y joignant des réflexions suggérées par la discussion de ces documens.

Le désir de propager le goût des sciences mathématiques, et de donner aux personnes qui s'en occupaient dans le royaume les moyens de faire connaître leurs recherches, fit naître l'idée de publier, sous forme de journal, la *Correspondance Mathématique et Physique*, 1825 †. M. Garnier prit part à la création de ce recueil; et deux ans après, il laissa le soin de la publication à son co-rédacteur. S'il est ici fait mention de ce journal, c'est parcequ'avec les *nouveaux Mémoires de l'Académie*, comme on le verra par ce qui suit, il renferme à peu près tout ce qui, dans ces derniers temps, a été écrit en Belgique sur les sciences mathématiques, du moins dans la vue de présenter des recherches originales ‡.

On aurait tort cependant de passer sous silence les dissertations qui, du temps du gouvernement précédent, ont été produites dans nos universités, soit pour l'obtention des grades de Docteur, soit pour les questions des prix annuels; quelques unes que j'aurai occasion de signaler présentaient un intérêt réel, et des observations utiles dont la science a tiré profit §.

Je ne crois pas devoir parler ici des ouvrages élémentaires: si la Belgique a produit peu du côté des ouvrages originaux, je ne pense pas qu'il y ait de pays qui puisse lui disputer la palme pour le nombre des traités d'arithmétique, d'algèbre, de géomé-

* Tome i. des *nouveaux Mémoires de l'Académie de Bruxelles*.

† Les rédacteurs avaient commencé par proposer des problèmes à résoudre dans les *Annales Belgiques*, où ils inséraient les solutions qui leur parvenaient.

‡ En parlant de ce recueil, il ne sera fait mention du reste que des savans Belges qui ont pris part à sa rédaction. Parmi les savans étrangers on distingue MM. Ampère, Babbage, Barlow, Bouvard, Chasles, Encke, Forbes, Gautier, Hachette, Hamilton, Sir J. Herschel, Horner, Lobatto, Plana, Poncelet, Potter, Pontécoulant, Capt. Sabine, Valz, Villermé, Whewell, etc.

§ Depuis 1830 on a aboli les concours et l'obligation pour les candidats au grade de Docteur d'écrire une dissertation.

trie, et de mécanique industrielle qui ont pullulé dans ces derniers temps, et qui se copiant les uns les autres, avec des prétentions à la nouveauté, n'avaient, la plupart du temps, de véritablement neuf que les erreurs qui y étaient introduites : quelques uns cependant doivent être distingués dans le nombre, et sont dus à des hommes qui avaient fait leurs preuves, et dont nous serons les premiers à reconnaître les mérites.

L'ordre des dates exige que nous parlions des recherches de M. Dandelin *, qui appartient au petit nombre des anciens élèves de l'Ecole Polytechnique qui, chez nous, ont continué de cultiver les mathématiques. Un écrit d'un ami le porta à revenir à une science qu'il avait abandonnée depuis longtemps. Dans cet écrit, qui a paru en 1820 †, se trouvaient entr'autres théorèmes sur les sections faites dans les cônes de révolution les suivans, dont quelques uns, faute de publicité suffisante, ont été reproduits depuis comme nouveaux, par différens auteurs qui y sont parvenus de leur côté. Je me bornerai à les énoncer pour l'Ellipse; on les modifiera sans peine pour la parabole et l'hyperbole.

1°. La différence des deux rayons vecteurs menés du sommet du cône aux extrémités du grand axe de l'ellipse vaut la distance des deux foyers de cette même ellipse.

2°. Si l'on joint un même point quelconque d'une ellipse au foyer de cette ellipse et au sommet du cône, la différence des rayons vecteurs est une quantité constant.

3°. La somme de deux rayons vecteurs menés du sommet du cône aux extrémités d'un même diamètre de l'ellipse est constante.

4°. La surface aplanie d'un cône à base elliptique est une ellipse qui a même excentricité que l'ellipse qui sert de base.

5°. L'aire d'un cône qui a pour base une ellipse, est à l'aire de cette ellipse comme la somme des rayons vecteurs menés du sommet aux extrémités du grand axe de l'ellipse, est à ce même grand axe.

6°. Tous les cônes qui ont pour base une même section conique, ont leurs sommets sur une autre section conique située dans un plan perpendiculaire à celui de la première, les foyers de l'une de ces courbes servans de sommets à l'autre, et réciproquement.

M. Dandelin, dans son mémoire *sur quelques propriétés de la focale parabolique* ‡, fit voir qu'on déduisait comme corollaire

* M. Dandelin d'abord officier du génie, avait passé ensuite dans l'enseignement ; il est rentré au service militaire en 1830.

† *Nouveaux Mémoires de l'Académie de Bruxelles*, tom. xi.

‡ *Id.*, tom. ii.

de ces propositions un théorème fort élégant qui se trouve aujourd'hui dans plusieurs ouvrages élémentaires, et qui peut s'énoncer ainsi : un cône droit étant coupé par un plan, on peut en général concevoir deux sphères qui, touchant le cône dans son intérieur, touchent aussi le plan sécant : alors les deux points de contact du plan et des sphères sont les foyers de la section conique. M. Dandelin a donné une nouvelle extension à ce théorème dans un mémoire *sur l'hyperboloïde de révolution et sur les hexagones de Pascal et de Brianchon* *, qui pour l'élégance des méthodes géométriques est peut-être le mémoire le plus remarquable que l'on ait écrit en Belgique †.

Enfin le tome iv. des *nouveaux Mémoires de l'Académie de Bruxelles* renferme encore deux autres écrits de M. Dandelin, l'un *sur les intersections de la sphère et d'un cône du second degré*, l'autre *sur l'emploi des projections stéréographiques en géométrie*.

M. Timmermans ‡ s'occupa également avec succès de la géométrie à trois dimensions ; on a particulièrement de lui des recherches ingénieuses sur la théorie générale des Caustiques qu'on a essayé dans ces derniers temps de réduire à sa forme la plus simple §, ainsi qu'un *essai sur une nouvelle théorie des courbes* ‖. L'auteur rapporte les courbes à un système dont les coordonnés sont les deux rayons de courbure successifs ; et leur équation est la relation qui existe entre ces deux rayons ; on conçoit que l'équation doit avoir ainsi plus de simplicité, puisque le paramètre de la courbe est le seul élément qui entre dans sa composition. Une partie du mémoire est consacré à faire voir l'utilité que l'on peut retirer de cette théorie, en la fesant servir à la résolution des equations numériques, à la recherche d'une classe d'intégrales définies, et à la résolution de quelques questions de mécanique.

Il est peu de pays où l'impulsion donnée à la géométrie à trois dimensions par l'illustre Monge ait laissé des traces plus sensibles qu'en Belgique. Les *Mémoires de l'Académie* et la *Correspondance Mathématique* en fournissent la preuve ; outre les écrits qui ont déjà été mentionnés, ils présentent un grand nombre de recherches de différens géomètres nationaux et étrangers

* *Nouveaux Mémoires de l'Académie de Bruxelles*, tom. iii.

† M. Bobilier s'est occupé du même sujet, tom. iv. p. 157 et suivantes de la *Correspondance Mathématique*.

‡ M. Timmermans, alors professeur de mathématiques, a passé dans l'arme du génie en 1831.

§ *Nouveaux Mémoires de l'Académie, Corresp. Mathém.*, et *Annales Mathématiques* de M. Gergonne, *passim*.

‖ *Mémoires de la Société des Sciences de Lille*.

dont les noms sont honorablement connus, tels que MM. Hachette, Poncelet, Chasles, Bobillier, Van Rees, Olivier, Reiss, Noël, etc.*

On demandera peut-être si cette tendance trop exclusive vers des méthodes qui n'ont dans le plus grand nombre de cas ni la généralité ni la richesse des méthodes analytiques ne doit pas être considérée comme dangereuse. Cette crainte paraîtra fondée sans doute surtout dans un pays où les mathématiques, nouvelles encore, ont besoin de s'établir sur un bon pied, et demandent à ne point être faussées dans leur direction. Cependant si cette tendance doit être restreinte en général, on aurait tort de vouloir l'arrêter chez ceux qui ont pour ce genre d'étude des talens particuliers. On n'a pas d'ailleurs tiré de la géométrie tout le parti possible, et il est des questions d'un certain ordre qui se laissent aborder par elle plus facilement que par l'analyse, et qui portent ainsi une conviction plus grande dans les esprits.

C'est pour appeler l'attention sur ce point scientifique que l'Académie, au Concours de 1830, avait demandé l'examen philosophique des différentes méthodes employées dans la géométrie récente, et particulièrement de la méthode des polaires réciproques. M. Chasles, de Chartres, à qui la médaille d'or a été décernée, a traité ce sujet avec beaucoup de talent, et a fait voir, dans un écrit qui ne tardera pas à paraître, que la plupart des théories nouvelles peuvent être déduites de quelques principes fondamentaux d'une fécondité remarquable, et qui sont pour la géométrie à peu près les analogues du principe des vitesses virtuelles pour la mécanique.

Ce n'était pas la première fois que l'Académie de Bruxelles mettait au concours des questions de géométrie à côté de celles d'analyse et de mécanique; elle avait proposé en 1824 une question sur la théorie des sections annulaires ou lignes spiriques, question d'une portée moindre que la précédente, mais qui cependant n'était pas indigne de fixer l'attention, puisque le tore trouve un fréquent emploi dans les arts. Le prix fut décerné à M. Pagani †, qui donna l'équation générale de ces courbes du 4e degré, et leur discussion complète avec les caractères pour les reconnaître. Après l'impression de son mémoire, l'auteur fit connaître, par la *Correspondance Mathématique*, un caractère

* M. Goebel, alors professeur de mathématiques à l'Université de Louvain, auteur d'un traité de géométrie, a publié un mémoire Latin sur les moyens les plus efficaces pour exciter les jeunes gens à l'étude de la géométrie descriptive. M. Goebel habite l'Allemagne depuis 1830.

† M. Pagani, depuis la suppression de la faculté des sciences de Louvain en 1830, est professeur à l'Université de Liège.

très simple pour distinguer la réalité d'une équation littérale du 4[e] degré assez compliquée; ce qui achevait la discussion des équations des sections annulaires.

*Analyse.*—L'analyse algébrique a été moins cultivée que la géométrie; cependant on peut citer quelques écrits qui renferment des choses remarquables, mais on trouve encore dans plusieurs une tendance à reporter l'analyse sur le terrain de la géométrie: ainsi MM. Dandelin, Timmermans et Van Rees ont puisé dans des constructions des méthodes nouvelles pour la résolution des équations*. On doit aussi à M. Van Rees† deux mémoires intéressans, l'un *sur l'analyse des fonctions angulaires*, l'autre *sur la convergence des séries et des produits continus*‡. Parmi les personnes qui ont cultivé l'analyse algébrique nous ne devons pas omettre non plus M. Verhulst, qui s'est particulièrement occupé de la théorie des nombres, et M. Noël, qui par son calcul des indices a essayé des voies nouvelles pour la solution de différentes classes de problèmes§.

Il existe encore des nuages dans la théorie de l'elimination. Quand quelques unes des racines de l'équation finale sont incommensurables, comme on ne peut en obtenir que des valeurs rapprochées, la substitution de chacune d'elles dans les deux proposées ordonnées suivant l'autre inconnue, en altère les coefficiens d'une manière qu'on ne peut apprécier, en sorte que chaque substitution dénature ou peut dénaturer les valeurs de la seconde inconnue, c. à d. lui en faire acquérir qui soient très éloignées des véritables. L'Académie de Bruxelles avait en conséquence demandé au Concours de 1823, de déterminer, sans résoudre effectivement les équations, 1. les limites extrêmes des valeurs de chacune des inconnues; 2. une limite au dessous de laquelle ne puisse tomber la différence entre deux valeurs de chacune de ces mêmes inconnues (ce qui rentre dans la méthode de Lagrange, pour la recherche des racines incommensurables des équations à une inconnue). L'Académie demandait de plus des applications numériques aux solutions réelles seulement, inégales, égales et incommensurables. Le mémoire qu'elle a couronné pour cette question se trouve inséré dans le tome iv.

* "Recherches sur la Résolution des Equations Numériques," par G. Dandelin, tome iii. des *Mém. de l'Acad.* "Sur les Limites des Racines des Equations littérales du 3[e] Degré," par M. Van Rees, tome v. *Corres. Math.* "Sur la Résolution des Equations Numériques," par M. Timmermans, tome ii. *Corr. Math.*

† M. Van Rees, alors professeur de mathématiques à l'Université de Liège, se trouve en Holland depuis 1830.

‡ *Corresp. Math.*, tome vi.

§ M. Verhulst, professeur à l'Ecole Militaire de Bruxelles; M. Noël, professeur à Luxembourg.

des *Mémoires Couronnés*; il est de M. Vène, officier du génie en France.

Les *Mémoires de l'Académie*, tome v., contiennent aussi un écrit intéressant de M. Pagani sur un point délicat d'analyse. Le développement d'une fonction arbitraire en séries trigonométriques, indiqué d'abord par Lagrange, et étendu ensuite par Fourier, servant à l'intégration des équations linéaires aux différentielles partielles, ne suffit pas dans tous les cas. L'objet du mémoire de M. Pagani est de transformer les fonctions arbitraires en séries, dont les termes généraux dérivent d'une certaine fonction plus générale que les fonctions symétriques, et comprenant celles-ci comme des cas particuliers.

Il serait difficile du reste et même superflu de s'appesantir ici sur les différentes recherches mathématiques qui ont été produites chez nous, surtout quand elles n'ont pas pour objet de faire avancer la science ou qu'elles ne marquent pas la tendance actuelle des esprits.

*Mécanique.*—Après avoir parlé des mathématiques pures, nous indiquerons les principaux travaux qui ont été faits dans ce qui se rapporte à la mécanique analytique. M. Pagani, qui semble avoir en vue de présenter plus tard un ouvrage qui résume l'ensemble de cette science, a successivement fait connaître, dans différens mémoires *, la manière d'envisager les théories fondamentales. Ainsi dans un premier travail sur le principe des vitesses virtuelles, il a donné une démonstration de ce principe, et le moyen le plus simple pour déterminer le déplacement virtuel d'un système invariable †. Il s'est occupé ensuite dans différens écrits de l'équilibre et du mouvement des systèmes flexibles, et il a été conduit ainsi à considérer l'intégration de différentes équations qu'on rencontre dans la théorie de la chaleur: par exemple, dans un mémoire couronné sur les mouvemens oscillatoires des systèmes flexibles linéaires ‡, M. Pagani fait voir comment une certaine intégrale définie employée par Fourier peut servir à déterminer les limites des racines d'une équation transcendante, et l'analogie entre les oscillations de certains systèmes linéaires et la propagation de la chaleur à travers certains corps solides.

Dans un autre mémoire, sur l'integration des équations relatives au mouvement de la chaleur dans les corps solides §,

* Voyez les *Mém. de l'Acad. de Brux.*, la *Corresp. Math.*, et le *Journal* de M. Crelle, vol. xii.

† *Nouv. Mém. de l'Académie*, tome iii., et une note dans le 11[e] volume du *Journal* de M. Crelle.

‡ *Mém. Couronnés de l'Acad.*, tome v. § *Mém.*, tome viii.

M. Pagani s'est proposé de résoudre par la méthode de Fourier les problèmes généraux qui comprennent comme des cas particuliers ceux qui ont été résolus pour la première fois dans la théorie de la chaleur. Cette solution était importante si l'on considère que des géomètres du premier ordre, et particulièrement M. Poisson dans le 19ème *Cahier de l'Ecole Polytechnique*, avaient combattu la méthode de Fourier comme insuffisante.

M. Timmermans, qui s'est occupé, comme M. Pagani, du principe des vitesses virtuelles* et du problème de la pression d'un corps qui porte sur plusieurs appuis, a présenté en 1829 à l'Académie de Bruxelles un mémoire sur la forme la plus avantageuse à donner aux aîles des moulins à vent. Dans ce mémoire, qui a obtenu la médaille d'or †, l'auteur a traité d'une manière très générale un problème qui déjà avait occupé plusieurs géomètres distingués, et les équations auxquelles il a été conduit vérifient dans les cas particuliers les résultats d'Euler, de Lambert, et de Luloss.

Il est à regretter que différens autres ouvrages de mécanique analytique remarquables sous plusieurs rapports n'aient point encore reçu de publicité; nous citerons en particulier deux mémoires de M. Timmermans sur lès pressions et torsions, un 3e mémoire du même auteur sur l'homme considéré comme agent mécanique, de même qu'un mémoire sur le zinc par M. l'ingénieur De Behr, où l'on trouve des théorèmes remarquables sur la résistance des solides.

L'Académie a couronné tout récemment (1835) un autre travail de mécanique pratique qui avait pour objet de déterminer le moyen le plus avantageux d'élever l'eau à des hauteurs de plus de cent mètres par le moyen de l'air atmosphérique. En proposant cette question, l'Académie n'avait pour objet que de provoquer de la part des hommes versés dans la science de l'ingénieur, une discussion approfondie sur une nouvelle application de l'air atmosphérique comme véhicule de la force motrice ‡.

La Belgique s'est associée dans ces derniers temps aux efforts des savans qui ont cherché à faire descendre de plus en plus parmi les classes industrielles les trésors scientifiques qui restaient trop exclusivement le domaine du géomètre. MM. Dandelin, Pagani et Lemaire ont été des premiers à seconder cet élan, soit par des cours publics, soit par des traités spéciaux de mécanique industrielle. Le Gouvernement, en 1828, avait de son côté commencé à Bruxelles un Musée des Arts et de l'Industrie, mais qui jusqu'à présent est demeuré comme un corps sans âme, comme un objet de pure curiosité, et, il faut le dire, plutôt

* *Corresp. Math.*, tome i. † *Mém. Couronnés*, tome viii.

‡ Le *Mémoire Couronné* est de M. Devaux, ingénieur à Liège.

comme un vaste magasin d'instrumens de physique anciens et modernes que comme un répertoire que l'on puisse présenter d'une manière utile à nos industriels. C'est un cabinet très curieux sans doute pour le vulgaire; mais où le physicien, à qui il semble plus particulièrement destiné, chercherait vainement pour les expériences délicates les instrumens dont il est dans le cas d'avoir besoin.

*Physique.*—La saine physique a fait trop peu de progrès en Belgique pour qu'on puisse même apprécier les déplorables lacunes qui viennent d'être signalées*. Dans l'Optique, qui est la branche pour laquelle on peut citer au moins quelques écrits remarquables, il se trouve cependant encore si peu d'adeptes qu'il est facile de compter ceux pour lesquels les phénomènes brillans de la polarisation ne sont plus un secret. C'était pour répandre davantage le goût de cette partie attrayante de la physique que M. Verhulst entreprit de donner une traduction de l'ouvrage de Sir J. Herschel sur la lumière; mais il fallut recourir aux presses de Paris pour en faciliter l'impression.

Vers la même époque M. Plateau publiait une *dissertation sur quelques propriétés des impressions produites par la lumière sur l'organe de la vue* †, dans laquelle il établissait, d'une manière beaucoup plus précise qu'on ne l'avait fait jusqu'alors, la durée des impressions produites sur la rétine pour les différentes couleurs; il examine dans le même mémoire, d'une manière générale, les illusions produites par des lignes qui tournent les unes devant les autres. Il revint à différentes reprises sur ces recherches ‡ qui avaient occupé vers la même époque MM. Roget et Faraday, et en suivant le cours de ces idées, il imagina de construire l'instrument ingénieux désigné tour à tour sous le nom de Fantascope, de Phénakisticope et de Stroboscope §.

Dans un autre mémoire dont la 1[ere] partie a été publiée dans le tome viii. des *Mémoires de l'Académie de Bruxelles*, M. Plateau présenta un essai d'une théorie générale comprenant la persistance des impressions de la rétine, les couleurs acciden-

* On pourrait reprocher peut-être à l'Académie de Bruxelles de ne pas avoir fait à la physique une part assez grande dans les programmes de ses concours. Cependant le 1[er] volume de ses Mémoires Couronnés contient un travail de M. De Hemptinne sur la vapeur d'eau employée comme moyen d'échauffement.

† Publiée à l'occasion de sa promotion au grade de Docteur, in 4°, à Liège, 1829.

‡ *Corresp. Mathém.* Voyez dans le même recueil, sur ce sujet, les recherches de M. Lefrançois.

§ Cet instrument d'optique fut construit presqu'en même temps à Vienne; et l'on peut présumer par le rapprochement des dates que l'auteur Allemand n'avait point connaissance de la publication faite à Bruxelles.

telles, l'irradiation, les effets de la juxtaposition des couleurs, les ombres colorées, etc. D'après cette théorie, lorsque la rétine, après avoir été écartée de son état normal par la présence d'un objet coloré, est subitement abandonnée à elle-même, elle regagne d'abord rapidement le point de repos; mais entraînée par cette espèce de mouvement, elle dépasse ce point et se constitue dans un état oscillatoire plus ou moins prolongé, d'où résulte la succession de deux sensations opposées, savoir, celle de la couleur primitive et celle de la couleur complémentaire. La première demi-oscillation constitue la *persistance de l'impression primitive*. D'un autre côté, pendant qu'une portion de la rétine est soumise à l'action de la lumière, les parties voisines participent à cette excitation jusqu'à une très petite distance, et donnent ainsi lieu au phénomène de l'irradiation; mais en vertu de la même loi de continuité, au delà de cette limite, se manifeste un état opposé, d'où résulte la sensation de la teinte complémentaire qui modifie la couleur des objets voisins. M. Plateau a montré que, plus loin encore, se retrouve quelquefois une légère nuance de la couleur primitive. Ainsi l'on a d'un côté relativement à *l'espace*, les mêmes phénomènes oscillatoires qui se produisent de l'autre relativement au *temps*: tous dépendent d'une même loi de continuité. Cette théorie est développée avec beaucoup de clarté, et repose sur des expériences dont plusieurs sont entièrement nouvelles.

M. Plateau a inséré, depuis, différentes notes sur la vision dans les *Bulletins* de l'Académie de Bruxelles, où l'on trouve aussi l'extrait d'un mémoire très intéressant de M. le Professeur Crahay *sur quelques phénomènes de vision*. Ce dernier physicien a exposé, d'une manière claire et très satisfaisante, comment les objets forment leurs images au fond de l'œil, et comment il faut s'expliquer une quantité d'illusions d'optique dont les physiciens ont parlé dans ces derniers temps; lui-même il produit plusieurs expériences nouvelles, et qui se déduisent comme conséquence de sa théorie. M. Crahay est conduit à conclure de ses recherches que l'œil présente à la fois, dans la formation des images, l'aberration de sphéricité et l'aberration de réfrangibilité, conformément aux recherches faites récemment encore par M. Powell.

En parlant de la partie de la physique qui concerne la vision, nous ne croyons pas devoir mentionner la théorie des caustiques dont plusieurs auteurs se sont occupés, parcequ'elle appartient plutôt aux mathématiques pures.

Les phénomènes de l'Electricité ont médiocrement excité l'attention des Belges; je ne parle point de ceux qui remontent au temps de l'illustre Volta, et qui ont trouvé place dans le *Journal*

*de Chimie* de M. le Professeur Van Mons, mais de ceux plus récens de l'Electro-dynamique, dont les développemens ont été si rapides et ont produit des résultats si surprenans. Le peu de personnes qui s'en sont occupées chez nous, se sont plutôt bornées à vérifier les résultats obtenus, et à présenter leurs observations sur les explications qui en étaient données; sous ce rapport, les recherches de MM. Lipkens, Glœsener, et Vanderheyden, qui a reconnu un des premiers l'effet des courans sinueux, n'ont pas été perdues pour la science. Cet état de choses pouvait tenir à la difficulté de se procurer de bons instrumens; cependant un jeune artiste Bruxellois, M. Sacré, dont le nom mérite de trouver place ici, construisait avec beaucoup de dextérité les instrumens les plus délicats, et souvent même avant qu'on put se les procurer chez nos voisins. En général, le travail des arts de précision est très négligé en Belgique, et mériterait des encouragemens. M. Sacré a construit des aimans remarquables par leur force; nous en citerons un de 27 kilogrammes de poids qui en a porté 196. Des aimans de 2½ kilogrammes ont porté 42½ kilogrammes ou 17 fois leur poids*.

Le Magnétisme terrestre, comme élément du temps, ne devait pas être négligé dans un observatoire; aussi, avant même l'achèvement de celui de Bruxelles, fut-il compris au nombre des objets qui devaient fixer l'attention de l'astronome. Cette partie de la physique avait été si honteusement négligée parmi nous, qu'en fouillant dans nos annales scientifiques on ne trouve pour les temps antérieurs à 1827 que trois observations de déclinaison faites à Luxembourg, à Nieuport et à Ostende, par deux physiciens Anglais. Depuis cette époque des observations régulières sur la déclinaison et l'inclinaison de l'aiguille ont été faites avec d'excellens instrumens de MM. Troughton et Simms de Londres. L'intensité magnétique relativement à d'autres stations fondamentales, telles que Paris, Londres, Berlin, etc., a été également déterminée avec soin et controlée par des physiciens distingués, et entr'autres par MM. Rudberg et le Capitaine Sabine. M. Forbes a fait à Bruxelles des observations comparatives analogues, mais les résultats n'en sont point encore connus.

La partie pratique du Magnétisme a conduit à des recherches de théorie qui ont été consignées dans un mémoire inséré dans les *Annales de Physique et de Chimie* (Juillet 1833), sous le titre, *Recherche sur les degrés successifs de force qu'une aiguille d'acier reçoit pendant les frictions multiples qui servent à l'aimanter.*

* M. Sacré a aussi essayé de construire des chronomètres, partie essentiellement négligée chez nous; et comment aurait-on pu cultiver l'horlogerie avec succès, puisqu'il n'y avait pas même les moyens de connaître l'heure.

La même lacune se fait remarquer chez nous dans tout ce qui tient aux températures intérieures de la terre ; il n'existait à notre connaissance aucune observation à ce sujet avant celles qui furent commencées à l'Observatoire de Bruxelles en 1834, et qui se poursuivent régulièrement au moyen de huit thermomètres placés à des profondeurs inégales entre la surface du sol et 24 pieds d'abaissement.

*Météorologie.*—Quant à la Météorologie, on trouvera un aperçu historique de ses phases en Belgique jusqu'à ce jour dans le tome i. des *Annales de l'Observatoire de Bruxelles*. Il résulte de cet aperçu que cette branche de nos connaissances est loin d'y avoir été cultivée avec succès, puisque pour la ville par excellence, pour la ville de Louvain, qui pendant quatre siècles a été en possession d'une Université, on ne connait pas une seule série d'observations *.

On peut même regarder comme nul tout ce qui avait été fait en météorologie avant la fondation de l'ancienne Académie de Bruxelles. Les relations que ce corps savant établit en 1784 avec la Société Météorologique de Manheim produisirent des observations intéressantes qui furent consignées dans les actes de cette dernière Société. Mais ces observations abandonnées après quelques années furent reprises à différentes époques par des particuliers avec beaucoup de zèle sans doute, mais généralement avec des instrumens défectueux. On doit distinguer parmi eux MM. Poederlé et Kickx qui observaient à Bruxelles. Cependant tous ces physiciens observaient avec des baromètres peu précis et dépourvus de verniers. Ils négligeaient les corrections des températures et de l'action capillaire, c'est dire assez combien peu la saine physique avait fait de progrès parmi nous. Un seul météorologiste vraiment digne de ce nom, M. Crahay, nous a donné une série d'observations faites à Maestricht avec d'excellens instrumens et qui remontent à l'année 1818†. On peut les ranger parmi les meilleures observations de ce genre que possède la science. M. Crahay, actuellement établi à Malines, a repris le cours de ses recherches météorologiques ; elles se lient à celles de l'Observatoire de Bruxelles qui en publiera régulièrement les résultats, comme ceux des observations qui se font simultanément, avec des instrumens comparés, au château de

* M. le Professeur Van Mons, qui appartient actuellement à l'Université de Louvain, et qui s'est occupé avec succès des phénomènes électriques, a publié dans le tome iv. des *Mémoires de l'Académie* un mémoire sur les brouillards de diverses natures ; il s'est aussi occupé de la théorie de la rosée.

† Voyez les différens volumes de la *Corresp. Math.* On trouve aussi dans le tom. viii. un mémoire intéressant sur les corrections à faire aux observations barométriques.

Rollé, près de Bastogne*, à Liège, à Anvers, et à Gand†. Ce système combiné d'observations fait d'après de bonnes méthodes et avec des bons instrumens, donne lieu d'espérer des résultats satisfaisans pour la science et nous mettra sans doute dans une meilleure voie que celle où nous avons été jusqu'à présent.

M. Crahay vient de présenter à l'Académie de Bruxelles un mémoire sur la variation diurne du baromètre, dans lequel il a obtenu pour principales conclusions :

1. En prenant les moyennes de trois années, l'instant du maximum arrive à $9^{h}\cdot259$ du matin, et celui du minimum à $3^{h}\cdot812$ de l'après midi ;

2. Pour les six mois Avril, Mai, Juin, Juillet, Août et Septembre, le maximum arrive de meilleure heure et le minimum plus tard que pour les six autres mois. Dans la 1ere période la durée de l'oscillation diurne est de $7^{h}\cdot68$ ; dans la 2eme elle n'est que de $5^{h}\cdot72$.

*Astronomie.*—Parlerai-je de l'Astronomie, qui couronne pour ainsi dire l'édifice des sciences, et qui pourrait donner la mesure de la hauteur à laquelle un peuple est parvenu à s'élever ? l'état d'abandon dans lequel elle est restée chez nous ne ferait pas augurer en notre faveur. Les seules observations que l'on ait faites depuis un siècle et demi, c. à d. depuis que l'astronomie a véritablement pris rang comme science, sont dues à un étranger, à M. Pigott, dont il a déjà été parlé. Frappé de cette lacune, et pressé par de vives prières, le gouvernement des Pays Bas, après deux années d'hésitation, ordonna en 1826 la construction d'un observatoire à Bruxelles, et il est juste de dire qu'il voulait dès lors le rendre digne de l'état actuel de la science, et plus riche même que ceux des Provinces du Nord. Dès l'année suivante il fit construire les instrumens par les artistes les plus habiles, MM. Troughton et Simms en Angleterre, M. Gambey en France, et M. Kessels, notre compatriote actuellement établi en Allemagne ; mais les travaux dont la Régence s'était chargée marchèrent avec la plus déplorable lenteur. La Révolution de 1830, comme nous l'avons déjà dit, faillit entraîner la ruine de l'observatoire, et détruire pour long temps encore l'avenir de l'astronomie en même temps que toutes les observations

* Les observations de Bastogne, faites avec beaucoup de soin et de zèle par M. Wautier, fils du sénateur, datent de 1831.

† A' Liège les observations ont été faites en 1830, -31, et -32, par M. Davreux (voyez le tome i. des *Annales de l'Observatoire de Bruxelles*) ; elles ont été continuées ensuite par M. Deville-Thiry.

Les observations de Gand et d'Anvers ne sont pas encore entièrement organisées ; elles seront faites dans cette dernière ville par M. Veyt, ancien membre de la Régence.

régulières qui s'organisaient. Mais cet orage s'est heureusement dissipé, sans que les constructions aient marché avec moins de lenteur. Aujourd'hui les instrumens sont terminés, et plusieurs viennent d'être mis en place.

J'ai tracé un tableau rapide de l'état des sciences physiques et mathématiques dans ce royaume. J'aurais désiré pouvoir vous entretenir aussi de l'état de la Chimie et des Sciences Naturelles, dont l'avenir parait beaucoup plus rassurant que celui des sciences exactes. La Géologie surtout a reçu une impulsion très heureuse, qui semble particulièrement due aux encouragemens de l'Académie de Bruxelles. Les Concours annuels ont fait naître une série de travaux importans sur la géologie de nos provinces; et bientôt l'on se trouvera à même de construire avec ces matériaux un travail d'ensemble qui pourra rivaliser avec ce que l'on a de mieux dans ce genre.

On a pu voir par ce qui précède que la nouvelle Académie n'est pas restée au dessous de sa mission. Les services qu'elle a rendus aux sciences historiques ne sont pas moins importans*; mais il est pénible de le dire, ses efforts ont été moins appréciés à l'intérieur que par les étrangers.

Il me reste à présenter une dernière observation. J'ai dit que les études profondes avaient été précédemment si négligées, que les hommes qui commençaient à s'y distinguer étaient jeunes encore, et que la plupart n'avaient point de carrière déterminée, quand arriva la Révolution de 1830. Pour ceux qui étaient initiés aux sciences mathématiques, l'avancement dans les grades militaires fut rapide; aussi l'on vit un grand nombre de Professeurs séduits par ces avantages quitter l'enseignement et prendre des grades dans l'armée; leurs élèves les plus distingués les y suivirent: or l'agitation de cette carrière et des études nouvelles à commencer les éloignèrent de leurs premières études. D'une autre part, le pays perdit plusieurs savans par l'état de délabrement des Universités, par la dépréciation du professorat, ainsi que par la suppression de deux des trois facultés de sciences qui existaient d'abord. Aussi l'on ne doit pas s'étonner de voir le peu de recherches mathématiques produites depuis cinq ans†; il n'en a pas été de même des sciences naturelles. La défection

* Le Gouvernement donnant suite à des travaux dont l'idée avait été suggérée par l'Académie, et dont l'exécution avait même commencé, a créé deux *commissions royales*, l'une pour la publication des manuscrits inédits, l'autre *pour les monumens du pays.*

† L'Académie de Bruxelles, depuis 1830, a publié six volumes de Mémoires, dans lesquels on ne trouve que trois mémoires mathématiques; et la *Correspondance*, qui ne comptait plus en Belgique que trois ou quatre collaborateurs, au lieu de trente au moins qu'elle en avait autrefois, a cessé de paraître.

n'a pas été aussi générale, et elle ne pouvait l'être ; c'est ce qui explique aujourd'hui leur état plus prospère.

On doit ajouter encore que, dans ce qui concerne les sciences exactes, l'opinion publique ne sert pas même de stimulant ; elle est trop peu éclairée dans ces matières, en sorte que l'état des sciences est, chez nous, comme un vrai tableau chinois (qu'on me permette cette comparaison), où tout est sur un même plan. Je m'estimerais heureux si cet essai, lu par mes compatriotes, pouvait contribuer à débrouiller un peu ce cahos, et à faire rendre justice au vrai mérite.

## *On the Phænomena of Terrestrial Magnetism: being an Abstract of the* Magnetismus der Erde *of Professor* Ch. Hansteen. *By Captain* Edward Sabine, *R.A., F.R.S.*

*M.* Hansteen*'s attention was first attracted to the subject of terrestrial magnetism by seeing in the year* 1807, at the Cosmographical Society of Upsala, a terrestrial globe, in the southern hemisphere of which was delineated an ellipse entitled "Regio polaris magnetica", having two foci, one near Van Diemen's Land entitled "Regio fortior", the other near Terra del Fuego entitled "Regio debilior". The "Regio magnetica" was stated on the globe to have been deduced by Wilcke from the observations of Cook and Furneaux. This magnetic system being at variance with the opinion which then generally prevailed, that the magnetic phænomena could be adequately represented by a single magnetic axis, M. Hansteen was induced to examine the observations referred to, and which he found fully to bear out the view which Wilcke had taken of them.

M. Hansteen proceeded to examine the observations which at that date had been made in the northern hemisphere. Those in the neighbourhood of Hudson's Bay sufficiently pointed out a "Regio fortior" in that vicinity, whilst those of the philosophers who visited the northern parts of the old continent in 1768 and 1769 to observe the transit of Venus, and of Schubert who visited Siberia in 1805, as clearly indicated the presence of a second point of magnetic attraction in the northern hemisphere, either in Siberia or in the sea to the north of it. This indication was further confirmed by the existence of a line of no variation in the vicinity of the White Sea; manifesting that the attraction of the needle to the westward by the point near Hudson's Bay must be counterbalanced by an attraction acting from the opposite quarter, drawing the needle in a contrary direction; the two attractions combined producing the intermediate direction of the needle, in a line coinciding with the geographical meridian.

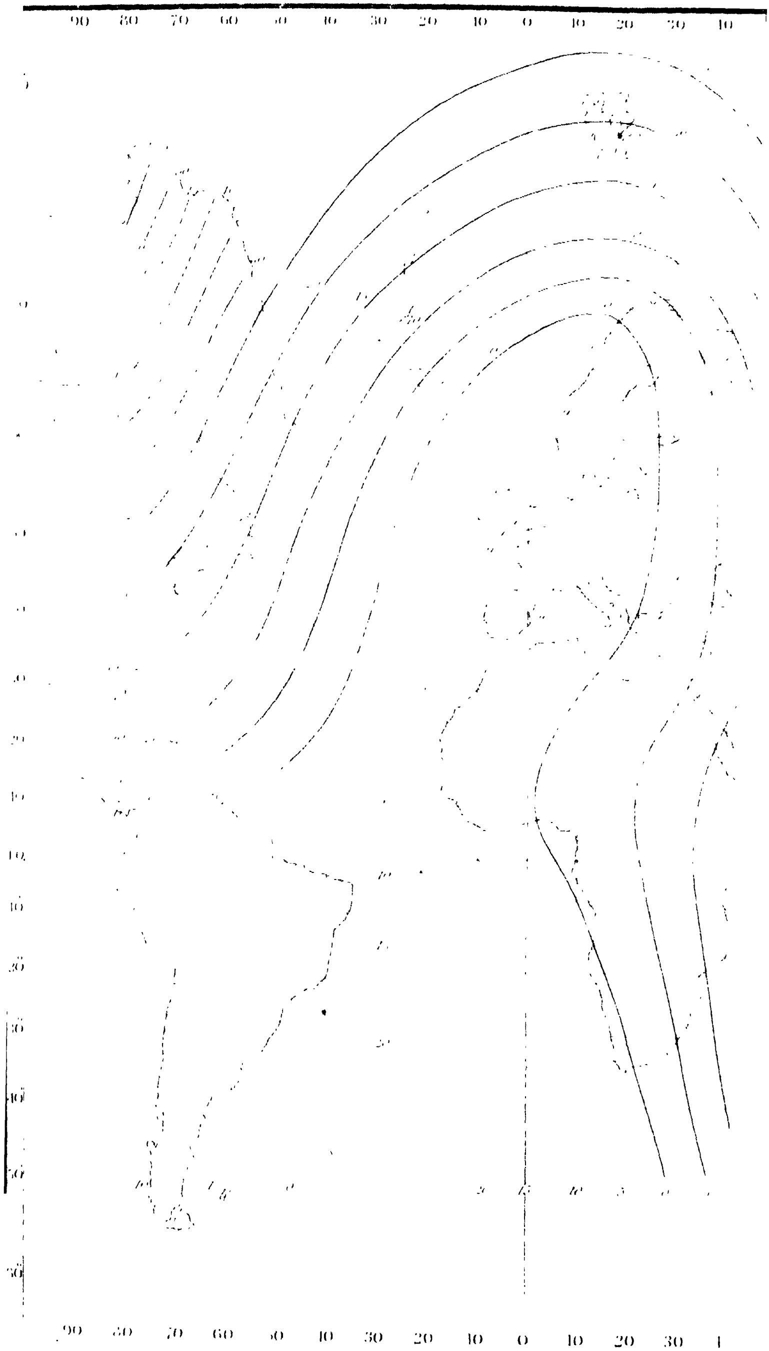
The points of strongest attraction in each hemisphere appearing in nearly opposite points on the globe, and the points of weaker attraction the same, M. Hansteen was led to connect them respectively together; and from circumstances relating to their motion, hereafter to be explained, he was induced to prefer an hypothesis of two magnetic axes, one stronger, and the other weaker, to Wilcke's hypothesis of elliptical magnetic regions.

On comparing the view which he had thus formed with Dr. Halley's magnetic chart of 1701, M. Hansteen found a general accordance. The principal differences were in regard to the situation of the Siberian pole, and to the motion of the axes, for both which Dr. Halley had very insufficient data. M. Hansteen considers therefore that Dr. Halley was the first person to discover the true magnetic arrangement of the globe, and that his deductions were fully as precise as the observations made in his time permitted. A century having since elapsed of observations with more perfect instruments and methods, M. Hansteen deemed that the time had arrived when their collection in one view and their careful examination might justify a far more complete and confident deduction. In 1811 the Royal Society of Sciences at Copenhagen proposed the following prize question: "In order to explain the magnetic phænomena of the earth, is one magnetic axis sufficient, or must we assume more?" The principal part of M. Hansteen's work was composed to meet this question, and received the prize: it was completed and published in the German language in 1819.

In an Appendix of 148 quarto pages are collected the various magnetic observations which had been made from the earliest times to the year 1817, and which were previously scattered in voyages and travels and in the works of philosophers and systematic writers. These are arranged in appropriate tables, and are the materials from which M. Hansteen has constructed maps of the variation corresponding to the years 1600, 1700, 1710, 1720, 1730, 1744, 1756, 1787, and 1800; and of the dip for the years 1600, 1700, and 1780. In collecting these observations and in arranging them in maps M. Hansteen has rendered a great service to all who desire to acquaint themselves with the facts regarding terrestrial magnetism that observation has made known.

In the first chapter, entitled "Of the Lines of Variation, and of their changes between the years 1600 and 1800," the authorities for the several maps of variation are discussed, particularly those on which the map of 1600 is founded; these are examined in considerable detail, and are shown to be fully deserving of confidence, and sufficiently exact for the purpose, notwithstanding the early period at which they were made.

In comparing the maps of 1600 and 1700 (Pl. I.), the difference appears at first sight so great that we can hardly imagine how one series of lines can have passed into the other. The interval of a century is certainly a longer one than is desirable, and it is greatly to be regretted that sufficient materials do not exist for an intermediate map. Aided, however, by the light thrown on



the changes in that century by the more exact knowledge we possess of those which took place in the succeeding century, we may still trace the general order of the changes between 1600 and 1700, though without the precision in point of dates which is subsequently attainable.

In the map of 1600 the line of no variation in the Atlantic quarter of the globe forms two branches, an eastern and a western. These bend towards each other at the point of their nearest approach in 20° to 30° north latitude; and we may for convenience divide this line of no variation into four portions; a north-eastern from Lapland to the middle of Africa; a south-eastern from the middle of Africa to Cape Lagullus; a north-western from Labrador to 30° north latitude; and a south-western from latitude 30° north, to the Pacific, passing across the northern part of South America. In comparing the maps of 1600 and 1700, the region of easterly variation included in 1600 between the two northern branches of the line of no variation appears to have moved subsequently in a north-easterly direction, towards the North of Asia, where it is seen in the maps of 1770 and 1787*. The westerly variation, which in 1600 occupied the coasts of Iceland and Greenland, moving south-westward, appears to have taken the place, in Europe and the adjoining seas, of the easterly variation which prevailed there in 1600. On the other hand, the region of easterly variation comprised between the two southern branches of the line of no variation of 1600 appears to have moved in a south-westerly direction towards the southern point of South America; and the westerly variation, which in 1600 occupied the Indian Ocean, to have moved correspondingly towards the Cape of Good Hope. Whilst the systems in each hemisphere were thus moving in opposite directions, the eastern and western branches of the line of no variation in 1600, approaching each other more and more in latitude 30° north, united previously to 1700, probably in the neighbourhood of the Cape Verd Islands, and appear in the map of 1700 as a continuous line, which they have ever since preserved. This is the line in Halley's chart entitled "Line of no variation in the Atlantic." The two northern branches, united between the 20° and 30° north latitude, and still comprising between them the small region of easterly variation, appear to have moved towards Siberia, where they are seen in the maps of 1770, 1787, and 1800; whilst the lines of westerly variation in the north-western Atlantic, following the eastward

* The Maps, which accompany this notice of M. Hansteen's work, are those of the Variation in 1600, 1700, 1741, and 1787: and of the Dip in 1600, 1700, and 1780.

movement of the line of no variation, advanced towards the east, until they joined the corresponding lines of westerly variation, moving in the opposite direction in Africa and the Indian seas; and the two united have appeared as continuous lines, from that time to the present.

If the map of 1700 be placed over that of 1600, and the points be marked in the latter in which the variation is the same in both, and a line be drawn connecting those points, two such lines will appear, one from Labrador across the Atlantic and the Brazilian continent, the other through the Persian and Arabian Gulfs and Madagascar.

The first of these lines is intersected near the middle by the line of no variation in 1600: the part north of the intersection is comprised within the region of westerly variation, and the part south of it within the region of easterly variation. On the east of the northern part the westerly variation increased between 1600 and 1700, and diminished on the west of it. On the east side of the southern part the easterly variation diminished, and increased on the west side. On the whole eastern side of the line of no change the magnetic direction became more westerly; the westerly variation increasing, and the easterly diminishing; whilst the converse held good on the western side. Hence it follows that the lines of westerly variation in the Atlantic turned on the points of their intersection with the line of no change, as on pivots, their motion being that usually termed "with the sun," causing the westerly variations to impinge successively on the North-west of Europe, and to advance progressively towards the south-east. It was thus that the line of no variation which was observed in London in 1657 did not reach Paris until 1666, nine years subsequent.

Ascending from particular to general conclusions, it may be stated as a general fact, that in the northern hemisphere the lines of variation collectively have an eastward motion, and in the southern hemisphere a westward motion.

In the map of 1700 the lines of no change between 1700 and 1756 are marked in a similar manner. In comparing the Atlantic line with its corresponding one, between 1600 and 1700, very little difference is perceived. The Indian line forms a pretty regular curve, from the Arabian Gulf and Madagascar to the 45th parallel of south latitude, and thence to the Straits of Sunda, and to China. At all places lying within the bend of this curve, the westerly variation diminished between 1700 and 1756, and increased in those lying outside of it.

In comparing the maps of 1787 and 1800, the westerly variation, which in 1787 occupied the eastern part of Asia, is found

to have moved in 1800 towards Corea and the adjoining seas; still evidencing the general progress eastward of the lines of variation in the northern hemisphere.

The second chapter is entitled "On the Lines of Dip, and on the Magnetic Force." The commencement of this chapter is occupied in discussing the materials existing for the construction of the maps of dip in 1600, 1700, and 1780. For that of 1600 the authorities, though few, are shown to be entitled to much confidence. Those for the map of 1700 are much more numerous; but that of 1780 is the first tolerably complete system of the lines of dip warranted by observations. A large space is occupied in examining the observations upon which the line of no dip, or, as it is frequently called, the magnetic equator, is laid down on this map. In the hypothesis of a single magnetic axis this line should be a great circle; it however differs much from the simplicity of figure which would correspond with that hypothesis. As much importance is attached to the correct delineation of this line, M. Hansteen inserts a table of the several observations, seventy in number, from which its course is laid down, and makes each observation the subject of a particular discussion.

The extreme southern latitude in which the line of no dip is found is $13\frac{3}{4}°$, which it reaches in from 20° to 26° west from Greenwich. From that point it slowly but uninterruptedly approaches the geographical equator to the east and to the west, until it cuts it in Africa in or about 25° east, and in the Pacific in or about 110° west. These points of intersection, or nodes, are thus not more than 135° apart, whereas if the magnetic equator were a great circle, they ought to be 180° apart. The intersection with the geographical equator in Africa is at an angle of $21\frac{1}{2}°$, but in the Pacific at an angle of only $7\frac{1}{2}°$. The greatest northern latitude it attains is in $12\frac{5}{6}°$ east of Africa in or about 65° east. Following its course from that point eastwardly, it slowly declines towards the south to the longitude of Malacca, where it coincides very nearly with the parallel of 9° north. Here, however, it bends again to the north, being found in $9\frac{1}{2}°$ north in the longitude of the Philippines; whence it finally descends without interruption till it cuts the geographical equator, as before stated, in the Pacific, in longitude 110° west. The greatest northern latitude attained by this line is in or about 65° east, and the southern extreme is in 23° west. These points are only 88° apart, furnishing additional evidence that the line of no dip does not correspond with a great circle on the earth's surface.

Each of the lines of dip and variation drawn in the maps

would have borne a detailed examination, in the same manner and to the same extent as is given of the line of no dip; but this would obviously have occupied too much space. M. Hansteen refers to the observations collected in the Appendix, as containing the authority for, and proof of, each line; adding his own assurance that an equally scrupulous care has been bestowed on all. Each line, separately considered, affords a distinct evidence of systematic inconsistency with the hypothesis of a single magnetic axis. The line of no dip has been selected as an example, because it is the line most usually referred to in such discussions.

A summary of the principal changes that have taken place in the dip in various parts of the world, from the earliest observations to the present time, is as follows: the north dip has increased in North America, diminished in Europe, and increased in eastern Asia and Japan; the south dip has decreased in South America, has been nearly stationary near the Cape of Good Hope, and has decreased in the vicinity of the Straits of Sunda and New Holland.

The remainder of this chapter is occupied in considering the few observations of the magnetic force that had been made when M. Hansteen's work was published. In the hypothesis of a single magnetic axis, it is a well-known law that the force should increase from the magnetic equator towards each of the magnetic poles, according to a certain function of the dip; consequently that all places having the same dip should have the same intensity. It is shown, however, that the observations not only do not accord with this law, but that they present marked and systematic differences from it. Comparative observations of the magnetic force in places in Europe and in America, having the same dip, show uniformly that a less intensity prevails in Europe than in America. In tracing along any of the lines of dip on which such comparative observations have been made, the intensity is found progressively to diminish from a maximum on the western side of America, to a minimum in the western parts of Europe, in those lines of dip which are included in the latitudes of Europe, and in somewhat more easterly meridians in those which approach the geographical equator, as well as in those further to the south. In tracing any of these lines still further to the eastward, the intensity again increases.

A second systematic difference from the law founded on a single magnetic axis is the following. In places lying under the same geographical meridian, a much greater increase of force corresponds to a given increase of dip in the meridians of New Holland and America than in those of Europe and Africa.

Hence M. Hansteen concludes that a more extensive acquaintance with the lines of equal magnetic intensity would show them to be equally irreconcileable with the hypothesis of a single magnetic axis as those of dip and variation are found to be*.

Having thus prepared and arranged the materials furnished by observation, M. Hansteen proceeds, in the 3rd chapter, to consider the evidence they afford of the number, situation, and movement of the magnetic poles.

The variation map of 1787 (Pl. I.) exhibits a tolerably complete system of the lines of variation. In the neighbourhood of Hudson's Bay, the indication of a point of directing influence is obvious in that quarter, evidenced by the characteristic circumstances of a rapid convergency of the lines, and of the proximity of great easterly and great westerly variations. A similar indication is seen to the south of New Holland. In two other quarters also, viz. to the south of Cape Horn and in Siberia, are less obvious, but still decisive, characteristics of the existence of points of directing influence.

In the map of the dip in 1780 (Pl. II.), the arrangement of the lines of dip corresponds to the indications thus traced in the lines of variation. In each hemisphere the lines of dip have a double flexure, those in the northern hemisphere making two loops to the southward, and those in the southern hemisphere making two loops to the northward. In the lines of dip also the directing influences in Siberia and south-west of Cape Horn are observed to be less distinctly marked than those in Hudson's Bay and New Holland.

In regard to the magnetic force, it has already been seen that

* In 1825 I published a series of observations which I had made in 1822 and 1823 on the magnetic dip and intensity at several stations comprised between the meridians of 76° W. and 23° E., and the parallels of 12° S. and 80° N. I was at that time unacquainted with M. Hansteen's work, having been little in Europe since its publication; but the irreconcileability of my observations with the hypothesis of a single magnetic axis was too striking to be overlooked, and was accordingly noticed by me: the direction of the curves of equal dip and of equal force was so far from corresponding, that the latter assuredly could not be computed, as had been supposed, by any function of the observed dip. The lines of equal dip crossed the geographical parallels of latitude at a small angle which nowhere exceeded a few degrees; whilst the isodynamic lines, within the space comprised by the observations, might be represented with tolerable approximation by concentric curves around an assumed maximum of intensity situated near Hudson's Bay. On becoming acquainted with M. Hansteen's work, I was much struck by the accordance of my observations, both of dip and force, with the system which he had anticipated from a study of the phænomena elsewhere; and I should not have failed to have noticed this circumstance publicly, had not M. Hansteen himself anticipated me in a review of my observations in the *Annalen der Physik*.

on lines of equal dip the intensity is greatest in America; that it diminishes in going eastward towards Europe and Africa, and again increases, as the line of equal dip is traced, still eastwardly, towards the centre of Asia and the Indian Ocean. Also, that in equal spaces on a meridian, greater changes of intensity take place in the longitudes of America and New Holland than in those of Europe. It is unnecessary to dwell on the accordance which these facts present with the inferences drawn from the configuration of the lines of dip and variation, of the action of four points of directing influence.

To obtain the approximate situations of these four points, M. Hansteen constructed two maps of the polar regions of the globe on a polar projection. (Plate 3.) On the south-polar map, the variations observed by Cook and Furneaux in their voyages of 1773 and 1774 are represented by arrows, indicating the angles made by the compass needle with the geographical meridian. On the north-polar map are represented, in like manner, the variations observed in the northern hemisphere by Cook, Phipps, Lowenhorn, Schubert, Billings, and others, from 1769 to 1805. The head of the arrow marks the place of observation, and its direction is that of the compass-needle, so that the angle which it makes with the meridian is the observed variation. In the map of the south-polar region, all the arrows between the meridians of 60° E. and 140° E. are perceived to have nearly an uniform convergency: their directions prolonged would all intersect somewhere about 135° E., and 69° or 70° S. latitude: and all the arrows comprised between the meridians of 240° E. and 320° E. are convergent to a second point of intersection, situated about 240° E. and 78° S.; whilst in the spaces intermediate between the meridians where the arrows are thus respectively convergent, the magnetic directions have no point of common convergence; the arrows prolonged do not intersect; all point intermediately between the two positions of directing influence; but the particular direction of each arrow appears to be determined by its relative proximity to one or other of the directing points, the influence of which predominates accordingly in the direction assumed by the compass-needle.

M. Hansteen computes by spherical trigonometry the latitude and longitude of the mean points of intersection of the two converging portions of the observations represented by the arrows: the one to the south of New Holland is in 136° 15′ E. and 69° 27′ S.; the other, to the south of Terra del Fuego, is in 236° 43′ E. and 77° 17′ S.; the observations employed in these deductions were made in the years 1773 and 1774.

Looking next to the map of the north-polar region, we find

again two points of convergence: one, of the observations in the northern parts of America comprised between the meridians of 230° E. and 290° E., which, computed by spherical trigonometry, is in 259° 58′ E. and 70° 17′ N., corresponding to the year 1769, and to a mean of all the intersections; the other, for the observations in the North of Asia, between the meridians of 70° E. and 130° E., in 101° 29′ E. and 85° 43′ N., corresponding also to a mean of all the intersections of observations of the year 1769. Here also the arrows in the intermediate spaces, viz. between 130° E. and 230° E., and between 290° E. and 70° E. (passing through 360°), have no point of common convergency; but their direction appears to be determined by one or other of the influential points, according to proximity.

We trace, then, by means of the polar maps four principal points of convergence in the direction of the magnetic needle; two in the southern hemisphere, designated by M. Hansteen A and *a*, and two in the northern hemisphere, B and *b*. Those in the southern hemisphere correspond to the year 1774, when A was situated in 136° 15′ E. and 69° 27′ S.; and *a* in 236° 45′ E. and 77° 17′ S. Those in the northern hemisphere to the year 1769, when B was in 259° 58′ E. and 70° 17′ N., and *b* in 101° 29′ E., and 85° 43′ N.

These points are called by M. Hansteen simply "points of convergence". Each is considered by him to mark the vicinity of a magnetic pole; the mode of deducing the positions of the poles from those of the points of convergence is a subject of discussion in a subsequent chapter.

M. Hansteen next proceeds to inquire whether these points of convergence appear to be stationary, or otherwise; whether they are equally the points of intersection of the directions of the compass-needle observed in the same localities, but *at other dates than those above stated.* The observations recorded in the appendix furnish him with the place of A corresponding to the year 1642; of *a* to 1586 and 1670; of B to 1725; and of *b* to 1594 and 1805. The result in every case shows that the points are *not* stationary; the earlier observations concur uniformly in making the intersections, in the northern hemisphere to the westward, and in the southern hemisphere to the eastward, of the later observations.

Neither the degree of exactness of the earlier observations, nor the intervals elapsed between them and the later ones, are sufficient to warrant any very decided inference of the periods in which each or any of the points would complete the circle of all the meridians. The periods indicated are, however, as follows: A 4609 years; *a* 1304 years; B 1740 years; and *b* 860 years.

It further appears that A and B recede from, and $a$ and $b$, on the contrary, approach, the geographical poles respectively adjacent to each.

We may conceive the poles corresponding to A and B (which are the points of more powerful directive influence in each hemisphere) to belong to one magnetic axis, and the poles corresponding to $a$ and $b$ to another magnetic axis, subject to the following conditions; viz. that the two northern poles have an eastward motion, and the two southern a westward motion; that in each axis the north pole has a considerably quicker motion than the south pole; that both poles A and B of the one magnetic axis recede from, and that both poles $a$ and $b$ of the other axis approach, the adjacent geographical poles; and lastly, that the axes prolonged are not diameters, but chords; they could only be diameters for a moment, on account of the opposite motion of the opposite poles; and then only if it should also happen that each end of the axis is at the same time at equal distance from its adjacent pole of the earth*.

In introducing the supposition of two magnetic axes, M. Hansteen carefully guards against being understood to affirm the existence of such, or as having any purpose beyond that of suggesting a convenient mode of connecting together and representing the facts made known by observation. So far from deciding on, he does not even discuss, the question, whether the causes of terrestrial magnetism are to be sought within the globe, or externally; though it is apparent that his own opinion inclines to the latter supposition. But with this understanding, he affirms that all the phænomena hitherto made known by observation admit of representation upon the supposition of two such magnetic axes as are above described; and he proceeds to exemplify this by showing the explanation such an hypothesis affords of the changes of variation and dip in the maps of those phænomena corresponding to different epochs.

Reckoning back the positions of the points of convergence to the year 1600, and viewing them in connexion with the map of the variation for that year, we should have $b$ in 83° N. and about 30° E., i. e. to the north-east of Spitzbergen; and $a$ to the south west of Terra del Fuego; at which time the axis $a\ b$ was consequently much nearer the Atlantic than the Pacific. B, requiring to be placed more westerly than its later situation, would be

* We may imagine in each axis two principal epochs; one when the two poles should be in opposite meridians, and the other when they should both be in one and the same meridian; but in the first case the prolonged axes would still not be diameters, because the opposite poles are at unequal distances, each from its adjacent pole of the earth.

found near Behring's Straits; whilst A, moving in a contrary direction, must be placed more to the east than by the later observations; and thus the axis A B will appear to have had predominant influence in the Pacific at that period. The easterly variation in the south part of the Atlantic would be chiefly due to the weaker pole *a*, then to the south-west of Terra del Fuego; and the westerly variation in the Indian Ocean to the stronger pole A, then south of Van Diemen's Land; whilst near Cape Lagullus the needle, equally attracted by both poles, would have, consequently, no variation. The westerly variation in Baffin's Bay would be due to the stronger pole B, then in the North-west of America. The easterly variation in Europe and in a part of the Northern Atlantic, as well as the westerly variation at Nova Zembla, to the weaker pole *b*, then north-east of Spitzbergen. Why the needle showed no easterly variation at Spitzbergen will not be perfectly explained till the theory of the dip is examined: had the two poles been of equal force, such a variation would have been found there.

At an epoch antecedent to 1600, *b* must have been still more to the west, near, for example, the east coast of Greenland; at which time we should infer that the easterly variation, which in 1600 overspread Europe, prevailed much further to the west: and this is accordant with the evidence afforded by the earlier observations of the variation at Paris; in 1541 the variation there was about 7° or 8° E.; in 1550 between 8° or 9° E.; about 1580 the easterly variation reached a maximum, which appears to have been about 11° 30′. From these facts we may conclude, that at a still earlier period than 1541, the easterly variation must have been less and less as we go further back, till about 1450, when it must have passed through zero into westerly variation. Turning now to the map of variation in 1600, we should say, in explanation of these phænomena, that in 1450 the north-east branch of the line of no variation passed through Paris; that the variation then became easterly, and increased till it reached a maximum in 1580, when it again decreased till 1666, in which year the north-west branch of the line of no variation passed through Paris. The pole *b*, continuing to move to the east, was followed by the whole system of easterly variations then observed in Europe, and which have now reached, and are found in the recent maps in, Siberia; whilst B, in the mean time, slowly approaching Europe, has caused an increasing westerly variation, which will hereafter decrease as B moves to the eastward towards the meridians of Europe. In the United States of America the decrease of the westerly variation will precede that in Europe; before another half-century B will have passed to the eastward of the

meridians of America, and the variation will have become, first zero, and then easterly.

As *a* moved south-westward in the Pacific, it must have been followed by the lines of easterly variation in the Southern Atlantic; and the westerly variation shown in the Indian Ocean in the map of 1600, must have progressed to the westward as *a* receded, and A advanced.

The motion of the small system of westerly variation from North-east Asia in 1770 and 1787, towards Corea and Japan where it was found in 1805, is explained by the eastern progress of *b*.

Our knowledge of the lines of variation in the Pacific is confined to recent dates: the phænomena, however, as represented in the map of 1787 and in subsequent maps, are in all particulars accordant with the explanation of them afforded by the hypothesis of two magnetic axes.

Viewing next the phænomena of the dip, we may infer that the south dip in South America decreases, because *a* is moving further into the Pacific; and the north dip in Europe decreases, because *b* is moving further eastward in Siberia. The great dips (from $84\frac{1}{2}°$ to $89\frac{1}{2}°$) observed by Hudson at and near the North Cape and Nova Zembla in 1608 were occasioned by the vicinity of *b*, which was, at that epoch, to the north-east of Spitzbergen. In Europe the dip will shortly again increase as B approaches our part of the world. The north dip in China increases, and the south dip in the same longitudes decreases, because *b* approaches the meridians of that quarter; and for the same reason the line of no dip, which was observed by Cunningham in the Chinese Sea in 20° north latitude in 1700, is now found considerably to the south of that parallel.

Proceeding next to consider the intensity of the magnetic force at different parts of the earth's surface corresponding to the two magnetic axes, we must first remember that the axes are supposed to be chords, and that in their present position they are both nearer to the surface of the Pacific than to that of the opposite hemisphere, i. e. than to the continents of Europe and Africa. A line drawn from the centre of the earth perpendicularly on the axis A B, and prolonged, would meet the earth's surface at a point in about 197° E., and near the equator. This would be the nearest point on the earth's surface to the middle of the stronger axis; and a point 180° from it, i. e. about 17° E., (on the continent of Africa, not far from the Bight of Benin,) would be the point on the earth's surface most distant from the middle of that axis; and here necessarily would be the minimum of intensity if this axis were the only one. But a line drawn from the earth's centre perpen-

dicularly on the weaker axis *a b*, and prolonged, would meet the surface in about 217° E.; this point would be the nearest to, and a point 180° from it, or about 37° E., near the east coast of Africa, would be the most distant from, the middle of the axis *a b*, and consequently about 37° E. would be the minimum of intensity if *a b* were the only axis. Hence it follows that the point of minimum intensity in the line of no dip resulting from both axes, must be somewhere in Africa between the two points of 17° E. and 37° E. From this point, then, we may imagine a curve to commence, passing northward through Europe, and southward through Africa, and cutting every line of dip at its point of minimum intensity. This curve, prolonged through all the lines of dip, would at length pass into the points where the dip is 90°, where the character of the curve would change from the curve of minimum to the curve of maximum intensity in the several lines of dip, which it would successively intersect till it again reached the geographical equator at some intermediate point between the meridians of 197° E. and 217° E., which are the points of greatest intensity of the two axes respectively, on the line of no dip.

At the date of publication of M. Hansteen's work there existed very few observations of the intensity with which to compare the system of intensities here presented. Those which did exist were, however, conformable to it. The intensities under equal dips diminished from the west side of America (beyond which, on the side of the Pacific, no observations had been made,) to the coasts of Europe and Africa; where the existence of a minimum must be supposed, since, in proceeding still further to the eastward, the force was again found to increase, under dips of the same amount.

In the fourth chapter, M. Hansteen passes under examination Euler's investigation of the mathematical theory of the lines of variation due to a single magnetic axis under various assumed conditions. Of these, the fifth case discussed by Euler is, when the poles of the axis are in different meridians, and at different distances from the poles of the earth. This case meets precisely the present conditions of both the axes in M. Hansteen's hypothesis. Having premised Euler's formulæ in this case, he employs them in calculating successively the lines of variation corresponding to each of the axes A B and *a b*, in the positions they are supposed to have occupied in the year 1769. These lines are delineated on maps of both hemispheres, exhibiting separately the variation corresponding to each axis. These maps are then compared with the map showing the actual phænomena in the year 1770; and the result of the comparison may be summed up as follows: 1st, The variation computed from the axis A B agrees

extremely well with the actual variation in the neighbourhood of Hudson's Bay and Straits, and in the Southern Indian Ocean between New Holland and the Cape of Good Hope; that is to say, in places which are in the immediate vicinity of one or other of the poles of that axis. 2nd, The variations computed from the weaker axis *a b* represent, but not so perfectly as in the preceding case, the variations observed in the neighbourhood of its poles in South America and in Siberia. Hence we perceive, that in those localities where the force of each pole might be expected respectively to predominate, Euler's lines of variation calculated for the axis of that pole accord with the phænomena. 3rd, The greater the distance that any point on the earth's surface is from the poles of either axis, the less the observations are represented by either system of lines taken separately. Thus, in the eastern hemisphere, we ought to have for the axis A B a line of 25° west variation, passing through Northern Spain, Southern France, Germany, Prussia, Finland, and Russian Lapland; whilst from the weaker axis *a b* we should expect an easterly variation of from 6° to 7° in Spain, 10° in Finland, and 12° in Lapland. The combined influence of both axes should then produce a variation, in Spain, between the limits of 25° W. and 16° E.; in Finland, between 25° W. and 10° E.; and in Lapland, between 25° W. and 12° E. Now the variation map of 1770 shows in Spain 20° W., in Finland 5° to 6° W., and in Lapland 0. The nearer either pole of the axis *a b* is approached, the more the observed variation differs from that which would be given by the axis A B, and approximates to that which is due to the axis *a b*. In the western hemisphere the line of no variation computed from *a b* passes south-west of the Californian Sea to the intersection of the meridian of 243° E. with the latitude 15° S., from whence its course is more southerly towards the pole *a*. In the map of the variation in 1770, there is an obvious relation in the configuration of the lines of variation in the Pacific to this line of no variation due to the axis *a b*. Nowhere on the line is the actual variation in strict accordance with it, the nearest approach being 2° E.; the difference is occasioned by the influence of the stronger pole A. To the westward of this line, if the axis *a b* acted alone, the variation would have been westerly, but the effect of the stronger pole predominates as it is approached. Near New Zealand A B would give between 20° and 25° E., and *a b* 15° W.: the map shows 15° E. At Behring's Strait A B would give a somewhat greater easterly variation than is shown by the map of 1770; and here the neighbourhood of the weaker pole *b* draws the north pole of the needle to the westward. On a close and careful examination, it will be found a general rule, that the

variations shown by observation fall between the limits assigned by the consideration of each axis taken separately. There are two exceptions to this rule, which are in Java, and from Mexico to the Isthmus of Panama. But these apparent anomalies are also capable of being explained, and disappear when a correction is introduced, which M. Hansteen points out in Euler's investigation, which in certain cases slightly affects the calculations here made in strict accordance with Euler's formulæ; and when the true magnetic poles are substituted in the calculation for the points of convergence, which have hitherto been considered as coincident with them.

In concluding this chapter, M. Hansteen remarks, that as the curves of variation computed on the hypothesis of two magnetic axes either represent well the actual phænomena, or assign the limits within which the observations are found to fall,—and as four magnetic poles sufficiently explain the double flexure of the lines of dip,—and as the alterations of variation and dip are fully explained by the motions above described of the four poles,—and as, lastly, the phænomena of the intensity indicate a double magnetic axis,—we may consider this hypothesis to be as well established, as a means of representing the phænomena, as any hypothesis whatsoever introduced in physical illustration.

Chapter fifth is entitled "On the Theory of Magnets." Having shown that when two magnetic points act on each other, their mutual action, whether of attraction or repulsion, is the product of the absolute magnetic force of the two points into some function of their distance apart, M. Hansteen proceeds to investigate the elementary laws which regulate the action of a linear magnet upon a magnetic point situated, first, in the prolongation of its axis; second, in the perpendicular passing through its centre, or its equator. The action depends in both cases, first, on the distance of the point from the centre of the magnet; second, (and particularly if the distance be inconsiderable in proportion to the length of the magnet,) on the distribution of the magnetic intensity in the magnet itself. These, therefore, form the subject of two elementary laws, deduced from experiments, which consist in drawing a small compass-needle from its line of repose in the magnetic meridian by a linear magnet, placed horizontally, at different distances in succession, from four to twelve times the half axis of the magnet, in a line through the centre of the needle perpendicular to the magnetic meridian, and in noting the displacements occasioned thereby in the direction of the compass-needle. The displacements so occasioned are then compared with computed expressions, in which the influence of the magnet is considered to vary inversely, first, as the distance itself;

second, as the square; and third, as the cube, of the distance of the magnetic point from the centre of the magnet: and in which the distribution of the intensity along the magnetic axis is considered to vary, first, as the simple distance of the particles from the middle point; second, as the square; and third, as the cube of that distance. It is shown by the comparison, first, as regards the distance of the magnetic point and the centre of the magnet, *that when the displacements* are computed in the inverse proportion of the simple distance or of the cube, they differ widely from those observed; but that when computed as the squares, the accordance of calculation and experiment is satisfactory throughout the series. Second in regard to the distribution of the intensity along the magnetic axis, that the agreement is best in these experiments when the magnetic intensity of the particles is taken as the square of the distance from the middle point of the magnet.

The experiments, therefore, indicate the following elementary laws, viz.

1. That the attractive force with which two magnetic points influence each other is inversely as the square of their distance apart.
2. That the force in the axis of a linear magnet increases as the square of the distance from the middle point; or, that the absolute intensity of each point in the axis is proportional to the square of its distance from the magnetic centre.

The first law is the same which was originally derived by Mayer from experiments communicated to the Royal Society of Sciences at Göttingen; it has been since confirmed by other philosophers, and is in full accordance with the experiments of M. Hansteen.

A corroboration of the second law is considered to be obtained from other experiments, subsequently related, in which two linear magnets were employed for the purpose of examining the laws of their mutual action. M. Hansteen also notices the experiments of Professor Steinhausen, which lead to the same inference. He concludes, therefore, that there is at least strong probability in favour of the second law; and as, moreover, that law is only of importance in small distances from the magnet, approaching contact,—and as in its application to the phænomena of terrestrial magnetism the distances are always so considerable as to render almost imperceptible the effect of differences in the distribution of intensity in its magnet itself, its adoption on this occasion cannot give rise to any material error, even if it should not ultimately prove to be the true law.

Proceeding then from these laws, M. Hansteen pursues the following investigations, based upon them, viz.

The line of repose of an infinitely small magnetic needle within the sphere of action of a linear magnet.

The dip of the magnetic line of repose towards the surface of a sphere, having in its centre an infinitely small linear magnet.

The same, the magnet being eccentric.

The situations of dip 0 and dip 90°, with the intermediate lines of dip, in the two cases, first, when the magnet is in the centre of the sphere; second, when it is eccentric.

The magnetic intensity, and the isodynamic lines in both the preceding suppositions.

The action of a magnet, being in shape a parallelogram, upon a magnetic point in the prolongation of its axis, and in its equator.

The action of a cylindrical magnet upon a magnetic point in the prolongation of its axis, and in its equator.

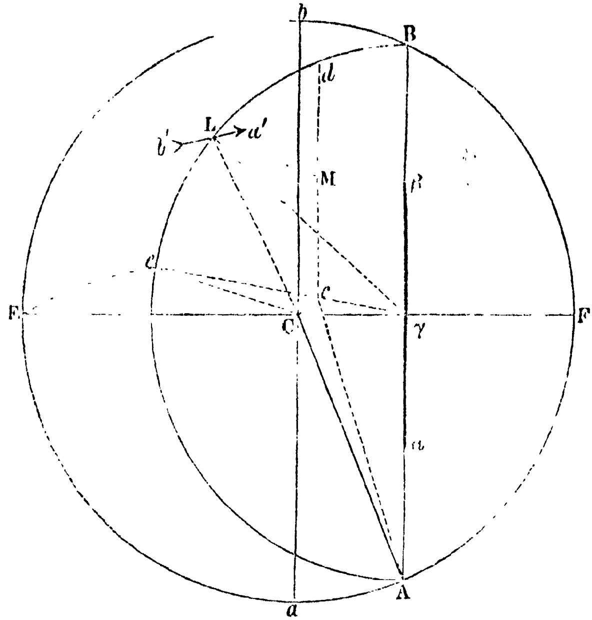
Upon these investigations are founded problems contained in the succeeding or sixth chapter, which is entitled "Application of the Theory of Magnets to the Theory of the Dip, Variation, and Force, at any given place of the earth's surface of known geographical position."

Suppose $\alpha\,\beta$, fig. 1., a single magnetic axis in the interior of the earth; its prolongation, till it meets the surface of the earth, forms A B, the magnetic chord, of which A and B are the extremities. It is possible that the centre of the chord $\gamma$, the mathematical centre of the magnet, and its neutral point (or the point in which the opposite forces are equal, and where there is consequently neither attraction nor repulsion), might be three different points; but they are at present considered to coincide all three in $\gamma$, the centre of the chord.

The circle E B F A is a great circle passing through C, the centre of the earth, and the magnetic chord. C $\gamma$ is the eccentricity of the chord. The magnetic equator is a great circle passing through the centre of the earth, perpendicular to the magnetic chord, and passing through its centre $\gamma$: its poles are $a$ and $b$, the extremities of a diameter of the earth parallel to the magnetic chord. If the magnetic axis were not eccentric, and the chord passed through the centre of the earth, its extremities A and B would coincide with $a$ and $b$, the poles of the magnetic equator. The radius of the earth being unity, the eccentricity C $\gamma$ is the sine of the arcs A $a$ and B $b$ which measure the distance between the ends of the magnetic chord and the poles of the magnetic equator.

E F is a magnetic diameter of the earth passing through $\gamma$; E is the point on the earth's surface most distant, and F the

Fig. 1.




point least distant, from the centre of the magnetic chord; or the apocentric and pericentric points.

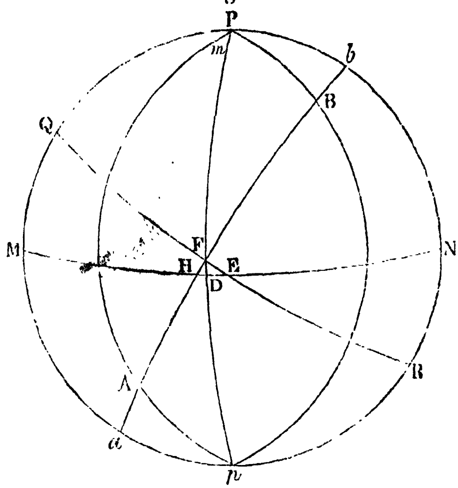
Every plane section of the earth passing through the magnetic chord is a magnetic meridian; all of which are small circles except the first, E B F A, which passes through the apocentric and pericentric points. A $e$ B is a magnetic meridian, and $e$ its point of intersection with the magnetic equator. The first meridian passes through the ends of the magnetic chord, the poles of the magnetic equator, and the apocentric and pericentric points. When the chord has no eccentricity there is no first meridian determined by nature.

Every plane passing through the axis of the magnetic equator is a magnetic vertical circle: every place has its own, and all are great circles. Were there no eccentricity in the magnetic axis, every magnetic meridian would be a magnetic vertical circle.

The magnetic polar colure is a great circle passing through the poles of the earth and those of the magnetic equator. The diametral colure is a great circle passing through the poles of the earth and the apocentric and pericentric points. Thus in

fig. 2, in which P*p* are the poles of the earth, P N *p* M is the polar colure, and P F *p* the diametral colure. A B and *a b* are

Fig. 2.



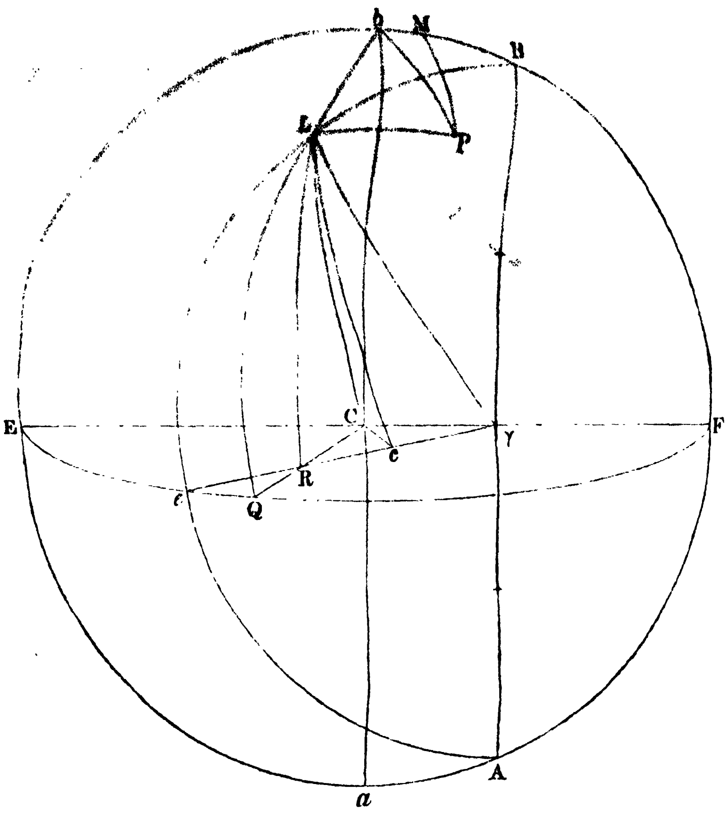

as in fig. 1, and the great circle passing through them is the first magnetic meridian. Q F R is the magnetic equator cutting the geographical equator M N in E, the pole of the polar colure; P B *p* and P A *p* are geographical meridians passing through the ends of the magnetic chord.

If in fig. 1. L be a place on the earth's surface of known geographical position, L $\gamma$ is its magnetic radius, or a line drawn from the place to the centre of the magnetic chord; L $\gamma$ *e* is its *true* magnetic latitude, or the angle formed by its magnetic radius and the magnetic equator; L $\gamma$ B is its true magnetic polar distance; E $\gamma$ *e* is its true magnetic longitude, or the angle between the magnetic meridian of L and the first magnetic meridian. B L *e* A being its magnetic meridian, a perpendicular C *c* to the magnetic equator from C gives *c* the centre of the meridian, C*c* its eccentricity, and L *c* its radius. L *c* is then the eccentric magnetic latitude measured at *c*, the eccentric centre.

In fig. 3. *b* L Q is a magnetic vertical circle through L, cutting the magnetic equator in Q C; L R is the intersection of *b* L Q with the magnetic meridian of L; the arc L Q is the apparent magnetic latitude of L intercepted on the vertical circle between L and the magnetic equator, or it is the angle L C Q; E Q is the apparent magnetic longitude, or the arc of the magnetic equator intercepted between E, the apocentric point, and the vertical magnetic circle passing through L; or it is the spheric angle E *b* L, or the plane angle E C Q.

The relations which these several quantities bear to each other, and the deduction, when they are known, of the angles which

Fig. 3.




the horizontal needle will make with the geographical meridian, and the needle freely suspended (the dipping-needle, for example) with the horizon of a place, are shown in the first six problems of this chapter. Problems 7 and 8 contain the deduction of the dip, variation, and force at any given point of the surface of a sphere which has two such magnetic axes, the geographical positions of which are known, as well as the proportion between their absolute forces. The 9th and 10th problems show the method of deducing the proportion between the absolute forces of the axes, when the situation and length of the axes are known, and either the dip or the variation is observed.

The expressions by which the values may be found of the

several quantities treated of in these problems, collected in one view, are subjoined.

In these expressions

$\alpha = \mathrm{A}\,a = \mathrm{B}\,b =$ the arc between the ends of the magnetic chord and the poles of the magnetic equator.

$\delta = \mathrm{P}\,b\,\mathrm{F} =$ the angle between the first magnetic meridian and the polar colure.

$\varepsilon = \mathrm{N\,E\,R} = \mathrm{P}\,b =$ the angle between the geographic and magnetic equators.

$\zeta = \mathrm{M\,P}\,b =$ the geographical longitude of the north pole of the magnetic equator.

$\eta = \mathrm{L\,C}\,c =$ the angle of the magnetic meridian with the horizon.

$\mu = \mathrm{L\,Q} = \angle\,\mathrm{L\,C\,Q} =$ the apparent magnetic latitude.

$\nu = \mathrm{E}\,b\,\mathrm{L} =$ the apparent magnetic longitude.

$\phi = \mathrm{E}\,\gamma\,e =$ the true magnetic longitude.

$90^\circ - u = \mathrm{L}\,\gamma\,\mathrm{R} =$ the true magnetic latitude.

$90^\circ - r = \mathrm{L}\,c\,e =$ the eccentric magnetic latitude.

$90^\circ - p = \mathrm{P\,L} =$ the geographical colatitude.

$q = \mathrm{M\,P\,L} =$ the geographical longitude reckoned east ward.

$i = d\,\mathrm{L}\,d'$ (fig. 1.) $=$ the oblique dip; or the angle of the magnetic line of repose and the tangent to the magnetic meridian.

$\omega = \angle\,d'\,\mathrm{L\,M}$ (fig. 1.).

$\varDelta = \angle\,b\,\mathrm{L\,P}$ (fig. 3.) the angle of the magnetic vertical circle, and the geographical meridian of L.

$\mathrm{R} = \mathrm{L}\,c =$ the radius of the magnetic meridian.

M and M′ $=$ the absolute forces of the two axes.

| | | | |
|---|---|---|---|
| M F $=$ the force <br> D $=$ variation <br> I $=$ the dip | at a given place due to one axis. | M′ F′ $=$ <br> D′ $=$ <br> I′ $=$ | the same quantities due to the other axis. |

e $=$ the angle of the forces of the two axes.

*D* $=$ the variation, *I* $=$ the dip, *K* $=$ the force — due to the compound action of the two axes.

### *Formulæ.*

1. $\sin\mu = \cos\varepsilon \,.\, \sin p + \sin\varepsilon \,.\, \cos p \,.\, \cos\,(q - \zeta)$.
2. $\cot\,(\nu + \delta) = \cos\varepsilon \,.\, \cot\,(q - \zeta) - \sin\varepsilon \,.\, \tan p \,.\, \operatorname{cosec}\,(q - \zeta)$.
3. $\cot\varDelta = \cot\varepsilon \,.\, \operatorname{cosec}\,(q - \zeta) \,.\, \cos p - \sin p \,.\, \cot\,(q - \zeta)$.
4. $\cot\phi = \dfrac{\sin\alpha}{\cos\mu \,.\, \sin\nu} + \cot\nu$.
5. $\mathrm{R} = \sqrt{1 + \sin^2\alpha + 2\sin\alpha \,.\, \cos\mu \,.\, \cos\nu}$.

6. $\cos \eta = \sin \alpha \,.\, \sin \Phi$.

7. $\cos u = \dfrac{\sin \mu}{R}$.

8. $\cos v = \dfrac{\sin \mu}{\sin \eta}$.

9. $\cot \omega = \dfrac{(A + B \cos 2\,u + C \cos 4\,u + \ldots) \sin 2\;)}{A' + B' \cos 2\,u + C' \cos 4\,u + \ldots} = \dfrac{Z}{N}$ *.

10. $i = v - \omega$.

11. $F = Z \,.\, \cos \omega + \omega N \,.\, \sin \; = Z \,.\, \sec \omega = N \operatorname{cosec} \omega$.

12. $\sin f = \tan \mu \,.\, \cot \eta$.

13. $\tan g = \tan i \,.\, \cos \eta$.

14. $\sin I = \sin i \,.\, \sin \eta$.

15. $D = \Delta - (f + g)$.

16. $\operatorname{cosec} c = \sin I \,.\, \sin I' + \cos I \,.\, \cos I' \,.\, \cos (D - D')$.

17. $K = \sqrt{M^2 F^2 + (M' F')^2 + 2\, M\, M'\, F\, F' \,.\, \cos c}$.

18. $\sin I = \dfrac{M\,F \;.\; \sin I + \;.\; M'\,F' \;.\; \sin I'}{K}$.

* (Formula 9). A, B, C, &c., and A′, B′, C′, &c., are coefficients supplied by the following table, according to different values of $\dfrac{\text{the magnetic radius}}{\frac{1}{2}\text{ magnetic axis}}$ or $\dfrac{L\gamma}{\beta\gamma} = \varrho$. If Q is the coefficient of the $\frac{1}{2}$ axis of the magnet to $r$ the earth's radius, then is $L\gamma = r\,R = Q\,R \,.\, \beta\gamma$; therefore $\dfrac{L\gamma}{\beta\gamma} = \varrho = Q\,R$.

| $\varrho =$ | 2 | 2·5 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | 0, | 0,0 | 0,0 | 0,0 | 0,00 | 0,00 | 0,00 | 0,00 | 0,00 | 0,000 | |
| A= | 10738 | 51527 | 29012 | 119711 | 60768 | 35018 | 21999 | 14716 | 10325 | 75216 | Numerator. |
| B= | 5292 | 16178 | 6308 | 14602 | 4738 | 1895 | 874 | 448 | 248 | 1461 | |
| C= | 1236 | 2416 | 653 | 849 | 176 | 49 | 17 | 17 | 3 | 14 | |
| D= | 287 | 360 | 67 | 49 | 6 | 1 | | | | | |
| E= | 63 | 52 | 7 | 3 | | | | | | | |
| F= | 13 | 7 | | | | | | | | | |
| A′= | 03855 | 18196 | 10104 | 040988 | 20619 | 11820 | 07402 | 04941 | 03461 | 25188 | Denominator. |
| B′= | 10969 | 52845 | 29647 | 121454 | 61374 | 35269 | 22118 | 14776 | 10359 | 75420 | |
| C′= | 2633 | 8153 | 3184 | 7357 | 2382 | 951 | 438 | 224 | 124 | 733 | |
| D′= | 609 | 1208 | 328 | 426 | 88 | 19 | 6 | 2 | 1 | 5 | |
| E′= | 140 | 179 | 34 | 25 | 3 | 1 | | | | | |
| F′= | 32 | 41 | 3 | 1 | | | | | | | |
| G′= | 7 | 4 | | | | | | | | | |

19. $\cot o = \frac{M\,F\,.\cos I}{M'\,F'\,.\cos I\,.\sin(D-D')} + \cot(D-D')$.

20. $D = D - o$.

The 7th chapter is occupied in an endeavour to assign more exactly the situation of the magnetic poles, the length of the magnetic axes, and their relative force in M. Hansteen's hypothesis.

Reverting to the four points of convergence found in the 3rd chapter, M. Hansteen shows that they are not identical with the situations of the ends of the magnetic chords. They would be so if the three following conditions were fulfilled, viz. 1st, If the earth had but one magnetic axis; 2nd, If the horizontal needle were always in the magnetic meridian; 3rd, If the magnetic meridians were all great circles. Unless these three conditions are fulfilled, the points of convergence must differ more or less from those points where the magnetic axis prolonged meets the earth's surface.

In the case of a single *eccentric* magnetic axis, the point of convergence belonging to each pole will fall in the first meridian between the extremity of the prolonged axis and the pericentric point. In the case of two such axes, the points of convergence will differ still more from the extremities of the chords: and the amount of difference will depend on the positions and proportionate forces of the axes. If the points of convergence are derived from observations made in the vicinity of the poles of the stronger axis, the deduction of the geographical situation of the magnetic chord may be made with the less uncertainty, because the intensity of that pole will predominate considerably over the influence of the weaker axis; but that axis will still have a sensible disturbing influence if, as is probable, the length of the $\frac{1}{2}$ axis of the magnet is less than half that of the earth's radius.

M. Hansteen here remarks that the study of the phænomena thus far has placed beyond question their entire inconsistency with a single magnetic axis; that it has further manifested their *general* accordance with such an arrangement of the lines of dip, variation, and intensity as would follow on an hypothesis of two magnetic axes; but it has given as yet no precise knowledge of any of the particulars of these axes except their number. Their exact situation, their length, and other dimensions, as well as the proportion of their forces, yet remain to be deduced. Their length is determinable were their situation and relative force exactly known. Their relative force would be deducible did we know their position and their length. But the preliminary determination of their situation is by no means easy to be accomplished. There are no less than eleven imper-

fectly known values, each of which influences the direction of the needle; and in the present still imperfect state of our knowledge of the phænomena themselves, it does not appear possible to determine *precisely* how great is the influence due to each. It is possible, however, to assign approximate values; and by comparing the results computed with these with the observed phænomena continually to approximate within narrower limits. It is true that a final determination must await more exact and multiplied observations; but those which have been already obtained are sufficient to show that the three phænomena of variation, dip, and intensity, observed indiscriminately over the whole surface of the globe, do admit of a very approximate representation, on the supposition of two magnetic axes, with such values as may at present be assigned.

To obtain such approximate values, the points of convergence found for the year 1775 were in the first instance assumed as the ends of the magnetic chords. Combining these with the dip observed in Hudson's Bay by Hutchins, and in Petersburg by Kraft, it appeared, after repeated trials, in which the length of the magnetic axis was taken successively as $\frac{1}{2}$, $\frac{1}{3}$. $\frac{1}{4}$, &c., to $\frac{1}{10}$ of the earth's axis, that were the positions of the points of convergence those of the ends of the magnetic chords, the proportion of $\frac{1}{10}$ of the earth's axis for that of the magnet would best accord with the dips observed in high magnetic latitudes: and as the result of calculations made with an axis so small in proportion as $\frac{1}{10}$ scarcely differs in the majority of instances from those made with an infinitely small axis, and as the latter supposition is the more convenient in calculation, it was thought preferable to assume it, until more correct places could be deduced for the ends of the magnetic chords. The values of $\alpha$, $\epsilon$, $\delta$, &c. were then deduced for both axes, as well as the pericentric point and poles of the magnetic equator for each axis. The geographical situation was then calculated of the two points where the magnetic equators would intersect each other; these points would necessarily be opposite points on the globe; and at them it is obvious that the dip, whether influenced by one or other axis, should be 0: consequently, without reference to the dimensions, the relative forces, or the eccentricity of the axes, and provided only that the mathematical and magnetical centres of each axis respectively are the same, there should be on the globe two points precisely opposite to each other where the dip should be 0; and two such points should accordingly be found on the observed line of no dip. Now if we refer to the map of the dip for 1780, we find that there are two points in the line of no dip opposite to each other on the earth's surface, viz. in

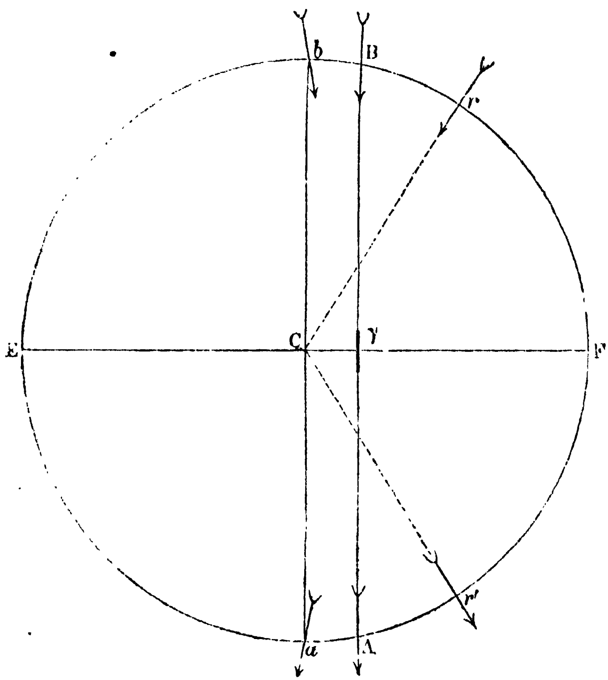
4° 20′ N. and S. latitude, and in 14° and 194° E. longitude. Calculated from the hypothetical elements above stated, they should be in 3° 55 N. and S. latitude, and in 6° 56′ and in 186° 56′ E. longitude. The differences 0° 25′ of latitude and 7° 4′ of longitude show that the errors of the elements of calculation are not very great even on the first approximation. We may here perceive the particular value which would attach to careful observations in the line of no dip in the vicinity of these two geographical positions; in the means afforded of correcting the situation of the poles of the magnetic equators.

M. Hansteen next proceeds to deduce more correctly the situation of the terminations of the magnetic chords, and to substitute these for the points of convergence hitherto employed instead of them. To explain this, let E B F A (fig. 4.) be a section of the earth in the plane of the first magnetic meridian, having an infinitely small magnet in $\gamma$, of which C $\gamma$ measures the eccentricity; A B is the prolongation of the magnetic axis or the magnetic chord; *a b* a diameter parallel to it; and *r r′* points where the magnetic line of repose is perpendicular to the surface, or where the dip is 90°. If the earth had but one magnetic axis, and that eccentric, the two points of the dip 90° would fall in the first magnetic meridian, each between the termination of the magnetic chord and the pericentric point F. Now it may easily be shown that when the eccentricity is sufficiently small to admit of the sine of the arc being taken for the arc itself, $B\,b = A\,a = \frac{1}{3}\,b\,r = \frac{1}{3}\,a\,r'$; or that the distance of the points where the dip is 90° from the poles of the magnetic equator is equal to three times the eccentricity. If now we imagine the arrows to represent the directions of the needle freely suspended, and further imagine the arrows to be brought down to a horizontal direction, as in the case of the compass needle by weighting the upper end, all the arrows between *b* and *r* will point in the direction *b r*, and all those between *r* and F in the direction F *r*; and in like manner the arrows between F and *r′* will point (though with the other end) in the direction F *r′*, and those between *r′* and *a* in the direction *a r′*; consequently the points of dip 90° (or *r r′*) will be also the points of convergence of the horizontal needle: and the arc $\alpha$ which measures the distance between the poles of the magnetic chord and the poles of the magnetic equator should be the arc *b* B or *a* A, and not the arc *b r* or *a r′*, which have been hitherto used for $\alpha$.

On the supposition of a single magnetic axis then the proper value of $\alpha$ could be easily derived from the value hitherto employed, of which it would be just one third. But the points of convergence obtained in the third chapter were not the points due to

the influence of a single axis alone: although derived from observations made very near to the respective points, still the ob-

Fig. 1.




servations were not entirely free, particularly in the vicinity of the points of the weaker axis, from the influence of the more distant axis. To obtain the points of convergence which should be due to each axis separately, M. Hansteen proceeded as follows: he selected certain of the most suitable observations of variation from which the situation of the points of convergence in the neighbourhood of the poles of the stronger axis were determined, and computed the disturbing influence on them of the weaker axis, employing for that purpose the approximate elements of the weaker axis as already stated (except that $\alpha$ was taken at one third its former value), and assuming the proportion of the absolute forces of the axes to be as 2 to 1. The direction of the needle that would be due to the influence of the stronger axis alone was thus obtained, and the intersections anew of the variations thus corrected gave fresh points of convergence, which if the elements of the weaker axis employed in the cal-

culation were perfect, would be the correct positions of the points of convergence due to the stronger axis. The position of that axis being in this manner approximatively gained, its corrected situation was employed, in conjunction with the variations observed near the poles of the weaker axis, in giving fresh points of convergence for that axis; the new position of which axis was in its turn again employed in recorrecting the variations observed near the stronger poles, and in producing a still nearer approximation to the position of the stronger axis. This alternating process was continued until the two last-found results exhibited no material difference.

The approximate places of the extremities of the magnetic chords having been thus found and substituted for the points of convergence, and the values previously calculated of $\varepsilon$, $\zeta$, $\delta$, &c. deduced afresh, M. Hansteen reverted to the observations of dip in the high magnetic latitudes, from which he had previously derived values for the length and proportionate forces of the magnetic axes, and selecting several of the most trustworthy of these observations, after various trials, he found that the observations were best represented when the length of each of the axes was taken at one third of the earth's axis, and the ratio of the forces as 1·7724 to 1.

With an axis of this length the points of 90° dip and of convergence of the horizontal needle would be nearer the end of the magnetic chord than in the case of an infinitely small axis; and by trial it was found that $\alpha = 0{\cdot}41\, b\, r$ suited best. Other small corrections, which were pointed out by the comparison of the calculation and observations, were also introduced in several of the elements.

Having reached this stage M. Hansteen considered that the elements were sufficiently corrected to admit of a more extensive comparison. He formed, therefore, a table of the most trustworthy observations of dip, variation, and intensity, made between 1787 and 1800, at eighty-four places, taken indiscriminately on the earth's surface, and divided into three portions, magnetic north polar, south polar, and equatorial. With the observations in this table, the dip, variation, and intensity computed for each of the eighty-four stations were compared, by which still further but very small corrections were introduced in the elements. The principal elements for computing the magnetic phænomena thus corrected are as follow:

| | Stronger Axis. | | Weaker Axis. | |
|---|---|---|---|---|
| $\alpha =$ | 3° 13′ | ........... | 5° 30′ | |
| $\varepsilon =$ | 29 0 | ........... | 28 28 | |
| $\zeta =$ | 291 51 | ........... | 95 58 | E. of Greenwich, |
| $\delta =$ | 129 49 | ........... | 46 40 | |

the magnetic axes being each one third of the length of the earth's axis, or Q = 3 ; and the ratio of the forces 1·7724 to 1.

In comparing the phænomena as calculated by the aid of the above quantities and as observed in the eighty-four tabulated stations, it is seen, 1st, with regard to the Variation,—that except at places in the immediate vicinity of the magnetic poles no discordance exists greater than about 5°. 2nd, In regard to the Dip, that the differences are generally inconsiderable, and mostly under 5°, except in a strip of the Atlantic extending from Teneriffe in a south-west direction to about 14° N. latitude and 313 E. longitude, and in a strip of the Indian Ocean extending from the Straits of Babelmandel to the Indian Peninsula. In the Atlantic strip the calculated north dips are from 10° to 11° too small, and in the strip in the Indian Ocean the calculated north dips are about 10° or 12° too great, and the south dips as much too small. 3rd, In regard to the Intensities, the observed and calculated agree well, except in the aforesaid strip of the Atlantic, where the force as well as the dip is made too small by calculation ; manifesting that the elements still require some correction, which they will best receive when more observations are obtained near the magnetic poles and along the line of no dip.

The eighty-four stations which have served for the above comparison extend over the most important parts of the earth's surface, both near the magnetic poles and the magnetic equator, and it is not probable that greater differences between the calculated and observed variations and dips will be found anywhere than those which appear in the table.

M. Hansteen remarks in conclusion that most of the differences would diminish, if not wholly disappear, by increasing the angle $\varepsilon$, which the equatorial planes of the two magnetic axes form with the geographical equator. By increasing the angle $\varepsilon$ for the stronger axis the northerly dip in the northern part of the Atlantic, and the southerly dip in the Indian Ocean would increase, and the northerly dip consequently decrease between the Red Sea and India. Further, the westerly variation in Musketto Cove and in the Indian Ocean, near the Cape of Good Hope, and the easterly variation between Van Diemen's Land and New Zealand would increase. By increasing the angle $\varepsilon$ for the weaker axis, the northerly dip in Petersburgh, Siberia, and Kamschatka, and the southerly dip near Terra del Fuego would increase ; and the westerly variation in Petersburgh and at the North Cape, and the easterly variation in Kamschatka would decrease, whilst the easterly variation near Terra del Fuego would increase. It is also probable that this alteration would increase the calculated intensities in the Northern

Atlantic. These are the parts of the globe where the principal differences take place between the calculated and the observed phænomena; but as the approximation has already been pushed sufficiently far to sanction the hypothesis, it is deemed unnecessary, and would probably be eventually time thrown away, to press to a nearer accordance, until the situation of the four points of convergence on the globe has been ascertained, with greater precision, by direct observation.

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Since the publication of the *Magnetismus der Erde* M. Hansteen has been engaged in personally determining the lines of dip, variation, and intensity, in the North of Europe, and throughout the Asiatic dominions of Russia. It is understood that he proposes to collect and embody, with the account of his own observations which he is preparing, all that has been accomplished by others since the early part of the present century; and thus to complete, in a second volume, the history of all that has been hitherto made known by observation concerning terrestrial magnetism. I have deemed it the more proper course, as well as that best fitted eventually to advance the inquiry, to await this publication from M. Hansteen, rather than to attempt, in this year's Report and with the materials which I now possess, or which are immediately accessible, the continuation of the condensed view which, by the aid of M. Hansteen's first volume, I have endeavoured to give of the results of observation in the two preceding centuries. The knowledge of the facts, conveyed by a suitably arranged view of what observation has made known, is a proper preliminary to an examination of the hypotheses proposed either to connect or to explain the phenomena.

It is a remarkable coincidence, and one of considerable importance towards a correct systematic knowledge of terrestrial magnetism, that at the same epoch at which the vicinity of the Siberian point of convergence has been visited by an observer of M. Hansteen's experience, furnished with the most perfect instruments, the other influential point in the northern hemisphere, in the North of America, has been also approached in various directions by the British officers employed in North-west discovery: and thus the position of the lines of dip, variation, and intensity in those two most interesting localities have been almost simultaneously ascertained, with an exactness heretofore unequalled. Those who have engaged in the endeavour to reduce to a common epoch observations made at intervals of time apart, can best appreciate how much of otherwise inevitable uncertainty is removed, when materials which should be rendered strictly relative to each other for the purpose of combination,

correspond in date. It is known that the north dip is at present diminishing in this part of the world about 3 minutes annually, and that it has not differed materially from that rate of diminution for several years past: but from a comparison of observations we learn, that in the Gulf of Guinea the annual diminution is little less than ten minutes, if it does not exceed that amount; whilst in the China Seas between the years 1700 and 1780 the north dip, on the contrary, increased, and at an average annual rate which could scarcely have fallen short of fifteen minutes, or a quarter of a degree a year. Our knowledge as yet is very far from being sufficient to enable us to render justly comparative the observations of different years, except in a very few parts of the globe.

In the northern hemisphere we probably now possess the requisite materials for describing the magnetic curves, from observations greatly to be relied on, and so nearly contemporaneous as to occasion but little error in reduction. But it is far otherwise in the southern hemisphere, particularly in what are usually called the high magnetic latitudes, and where an acquaintance with the facts would be of principal value towards a knowledge of the system of Terrestrial Magnetism. The enterprise of our merchant seamen has shown that these latitudes are far more accessible, in certain meridians at least, than had been previously supposed. The magnetic observations of the voyages of Weddell and Biscoe have been confined to those of the variation; these fully confirm M. Hansteen's position of the general westward movement of the lines of equal variation, in the southern hemisphere. But it is in the meridians left untouched by those vessels,—in those which include and are adjacent to those magnetic foci in the southern hemisphere, which M. Hansteen has called points of convergence, that observations would be chiefly useful; and observations confined to the variation, but including also the dip, and intensity of the force. The ice itself, or such lands as might be discovered by a vessel coasting the southern ice between the meridians of 80° E. and 260° E., would furnish the requisite localities for the observations of the three phænomena; and would supply what is wanting to complete a map exhibiting the arrangement, corresponding to a definite epoch, of the curves of equal dip, variation, and intensity, over the whole surface of our globe.

# RESEARCHES

## UNDERTAKEN AT THE REQUEST OF THE ASSOCIATION.

### *Report on the Comparative Measurement of the Aberdeen Standard Scale. By* FRANCIS BAILY, *Treas. Royal Society, &c.*

AGREEABLY to the request of the British Association I have compared the Aberdeen standard scale with the standard scale of the Royal Astronomical Society: but, as in a matter of this nicety and importance I did not wish the results to depend on my own comparisons only, I obtained the assistance of Mr. Bryan Donkin, Lieut. M. Johnson, Mr. Thomas Jones, and Mr. William Simms, all conversant with and much versed in micrometrical measurements, and who kindly lent me every assistance in their power.

The centre yard of the Aberdeen scale was chosen as the object of comparison with the centre yard of the Royal Astronomical Society's scale; that being the portion of the latter scale which had been directly compared with the Imperial standard yard prior to its loss by fire, at the destruction of the two houses of Parliament in November last. The following are the results of 56 comparisons made by the several parties above mentioned, and estimated in divisions of the micrometer microscopes (each division denoting $\frac{1}{20000}$ of an inch,) and show the number of such divisions by which the centre yard of the Royal Astronomical Society's standard scale exceeds the centre yard of the Aberdeen scale.

| 1835. | No. of Comp. | Divisions of Micr. | Temp. | Observers. |
|---|---|---|---|---|
| Feb. 16. | 8 | 26·56 | 48°·6 | Baily. |
| — 18. | 8 | 27·74 | 46·4 | Donkin. |
| — — | 16 | 29·16 | 48·9 | Donkin and Johnson. |
| — 19. | 8 | 27·09 | 45·8 | Jones. |
| — 20. | 8 | 27·65 | 45·3 | Simms. |
| — — | 8 | 26·93 | 46·2 | Baily. |
| Mean = | 56 | 27·71 | 46·9 | |

By which it appears that, from the mean of 56 comparisons, the centre yard of the Aberdeen scale is ·001385 inch (or about $\frac{1}{720}$ of an inch) shorter than the centre yard of the Royal Astronomical Society's scale, at the temperature of about 47° of Fahrenheit's thermometer.

I have also myself made 16 direct comparisons of the whole length of the same scales, namely 5 feet; and have obtained the following results, viz.

| 1835. | No. of Comp. | Divisions of Micr. | Temp. | Observer. |
|---|---|---|---|---|
| July 23. | 8 | 39·62 | 74°·6 | Baily. |
| — 24. | 8 | 39·24 | 72·7 | |
| Mean = | 16 | 39·43 | 73·6 | |

By which it appears that, from the mean of the 16 comparisons, the whole measure of the 5 feet Aberdeen scale is ·001971 inch (or about $\frac{1}{500}$ of an inch) shorter than the whole 5 feet of the Royal Astronomical Society's scale, at the temperature of 73°·6 of Fahrenheit.

I beg to add that the Aberdeen scale is in very good condition, and in excellent preservation: it appears to have been exceedingly well finished, and is by far the best of any that I have seen of Mr. Troughton's execution. And although the above results show a greater discordance from the correct measures than is desirable, yet as perfect accordance is seldom or never attainable, no inconvenience can arise from this circumstance, now the amount of the error is ascertained, and will consequently be known to those parties who may, at any future time, have occasion to make use of this scale.

Independent of the value of these experiments in thus determining the comparative length of the Aberdeen scale, they are important in a general point of view, in as much as, coupled with others that I have made with a similar object, they evidently show that the too prevalent notion "that standard scales, "made from one and the same prototype agreeably to Mr. "Troughton's method, would accord with each other," is not strictly correct. Indeed, it is now too evident (as I shall at some future time show more in detail) that a great number of minute, yet important, circumstances have hitherto been neglected in the formation of such scales; and without an attention to which, they cannot be expected to accord with that degree of accuracy which the present state of science demands.

## *Impact upon Beams.* *By* EATON HODGKINSON.

THE object of the present paper is an inquiry into some of the effects of impact upon beams when struck by bodies of different weight, hardness, and elastic force; and to compare theory with, and endeavour to adapt it better to, the results of experiment. The paper is a continuation of some experimental researches on the collision of imperfectly elastic bodies, which were published in the *Fourth Report of the Association*; and it is intended to contain proofs of the principal statements made in a short communication read at the Cambridge Meeting.

The preliminary conclusions, and some calculations with their results, will first be given; and afterwards the experiments, to which constant reference will be made for proofs and illustrations.

With the castings and every assistance in making the experiments I have been supplied, as on former occasions, through the liberality of Mr. Fairbairn, engineer, of Manchester.

### Conclusions from Experiments, &c.

Conclusion 1.—If different bodies of equal weight, but differing considerably in hardness and elastic force, be made to strike horizontally with the same velocity against the middle of a heavy beam supported at its ends, all the bodies will recoil with velocities equal to one another.

This is shown by the experiments on the 3rd beam, in which two balls $8\frac{1}{2}$ lbs. each, one of lead and the other of cast iron, suspended as pendulums, were made to fall through equal arcs against the middle of the beam, $13\frac{3}{4}$ lbs. weight between the supports; and the recoil of the leaden ball was nearly the same as that of the iron ball, though the hardness and elasticity of the two balls were widely different. This equality of recoil in the two balls was likewise shown to exist whether both fell through a small or large arc.

To vary the weight and quality of the balls, two were used, half the weight of the former, $4\frac{1}{4}$ lbs. each, one of lead and the other of bell-metal. In these, as before, when both were let fall through equal arcs, whether great or small, the recoil of one ball was nearly equal to that of the other.

To vary further the experiment on this beam, three balls were

used, about $\frac{1}{11}$ of the weight of the first, 9 oz. 7 drs. each, one of lead, one of bell-metal, and the other of hardened steel. The recoils from equal impacts in all these, though very anomalous, tended toward equality as before.

The beam here used was of steel, but that did not affect the results; for the same equality in the recoils will be found in the experiments on the 2nd beam, which was of cast iron.

Conclusion 2.—If, as before, a beam supported at its ends be struck horizontally by bodies of the same weight, but different hardness and elastic force, the deflection of the beam will be the same whichever body be used.

This conclusion is proved by the experiments upon the 2nd and 3rd beam, and with the same generality as in the former case. In those on the 3rd beam two balls $8\frac{1}{2}$ lbs. each, one of lead and the other of cast iron, were made to strike the beam with velocities varying from 1 to 5; and the deflections from equal impacts by the two balls are as below:

| Velocity of Impact. | Deflection from Leaden Ball. | Deflection from Iron Ball. |
|---|---|---|
| 1 | ·12 | ·42 |
| 2 | ·82 | ·88 |
| 3 | 1·23 | 1·26 |
| 4 | 1·66 | 1·69 |
| 5 | 2·12 | 2·11 |

In the impacts with the $4\frac{1}{4}$ lbs. balls of lead and bell-metal, the deflections from equal impacts, and varying in velocity from 1 to 6, are as below, and nearly equal:

| Velocity. | Deflection from Leaden Ball. | Deflection from Bell-metal Ball. |
|---|---|---|
| 1 | ·29 | ·31 |
| 2 | ·60 | ·62 |
| 4 | 1·12 | 1·12 |
| 6 | 1·73 | 1·65 |

The same equality is shown too, though with greater anomalies than above, in the deflections from impacts with the balls of lead, bell-metal, and hardened steel, 9 oz. 7 drs. each.

Conclusion 3.—The quantity of recoil in a body, after striking against a beam as above, is nearly equal to (though somewhat below) what would arise from the full varying pressure of a perfectly elastic beam as it recovered its form after deflection.

The fact in this Conclusion was sought for, because it seemed doubtful whether a bent beam would straighten itself with any nearer approach to the velocity arising from perfect elasticity than that given by the defective elasticity of the material of which it is made. Thus, two solid bodies of cast iron struck against each other recoil with only $\frac{7}{10}$ of their velocity of im-

pact, as appears from our experiments on the collision of imperfectly elastic bodies (*Fourth Report of the Association*). But a cast iron beam throws back a ball with a velocity much more nearly approaching to what would arise from perfect elasticity. This will be seen by comparing the observed results with the calculated ones in the experiments upon the 1st and 2nd beams, the calculated results being obtained from problem 1 following, where the beam is assumed to be perfectly elastic.

Conclusion 4.—The effect of bodies of different natures striking against a hard flexible beam seems to be independent of the elasticities of the bodies, and may be calculated, with trifling error, on a supposition that they are inelastic.

If the calculation be formed on a supposition that the time of the collision, in the first approach of the impinging body, is small compared with the time of deflection of the beam, and that the beam and striking body both proceed together afterwards as one mass (as is done in our following problems); the calculated deflections are somewhat greater than the observed ones, the difference sometimes amounting to one fifth or one eighth, as will be seen from the experiments on the 1st, 2nd, and 3rd beams. But the observed deflections in our experiments, excepting, perhaps, those on the 3rd beam, must be rather too small, arising from the resistance of the clay, into which a peg used for measuring the deflections was driven by the impacts.

This fourth Conclusion must only be admitted when there is nothing struck upon but the beam. When there is any other heavy body intervening between the striking body and the beam, touching the latter, and which must be struck before the beam can be deflected, then the elasticities of the concurring bodies exhibit their influence, and the result is greater than that obtained by calculation as above. This might be expected; and it is shown by the experiments on the 4th beam.

Dr. Young, in his *Natural Philosophy*, and Mr. Tredgold, in his *Treatise on the Strength of Cast Iron*, reason on this subject as if the striking body were inelastic, and we have here shown that this may be assumed, whatever the hardness and elasticity of the striking body may be*; or, probably, its weight with respect to that of the beam.

* Of this curious fact the Author would beg to suggest the following as a possible explanation. In the first moment of the impact upon the middle of the beam, each half of it, if its ends were not fastened, would have a tendency to turn round its centre of oscillation, or a point two thirds of the distance from the middle to the end, which is seen in experiment by the ends springing up after a blow. But it is probable that, besides this, the whole beam is thrown by the blow into a state of nodal vibrations, like as in a musical chord; there

In the elaborate paper on the "Measure of Moving Force," by Mr. Ewart, (*Manchester Memoirs*, vol. ii., *second series*,) there are, among other important matters, some ingenious inquiries respecting impact and the force of springs. These, however, do not appear to be easily applicable to our present subject generally; and the clay used by Mr. Ewart, in his experiments with pendulous balls, was the resisting medium, while in our case it was employed only to indicate the deflections.

Conclusion 5.—The power of a uniform beam to resist a blow given horizontally is the same in whatever part it is struck.

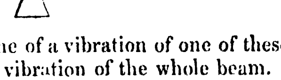
From the experiments on the 5th, 6th, and 7th beams, it appears that the beams, when supported at the ends, required the same blow to break them, whether they were struck in the middle, or half-way between that and one support. From a future investigation, too, it appears that the same is the case wherever the beam is struck.

Conclusion 6.—The power of a heavy uniform beam to resist a horizontal impact is to the power of a very light one as half the weight of the beam, added to the weight of the striking body, is to the weight of the striking body alone.

This is shown by Cor. 1. Prob. 2; for, from Cor. 2, the inertia appears to be half the weight of the beam; and the greater resistance of a large mass than of a small one may be inferred from the experiments on beam 4, and others.

Conclusion 7.—The power of a uniform beam to resist fracture from a light body falling upon it (the strength and flexibility of the beam being the same,) is greater as its weight increases, and greatest when the weight of half the beam, added

being one, two, or more nodes on each side of the middle. This will be understood from the adjoining figure, which represents the beam, when bent by an impact from the ball A, and the small excursions of the parts between the nodes.



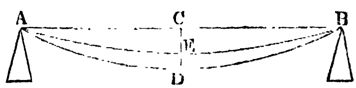
The time of a vibration of one of these parts is very small compared with the time of a vibration of the whole beam. Chladni has shown that if a uniform rod have its ends supported and be put into a state of double vibration as above, the number of nodes being $n$, there will be $(n+1)^2$ of these secondary vibrations for one whole vibration of the rod (Biot, *Traité de Physique*, tom. ii., p. 77–8). Hence, after the first concussion of a ball upon a heavy beam, the ball and beam in proceeding together are not constantly in contact, or in a state of equal pressure if they are. Their connexion appears to be a series of small impacts, or of approaches and retreats, the intervals between each of which are the time of one of these secondary vibrations. And during these intervals it is presumed that the compressed surfaces of the ball and beam recover themselves after the first concussion, leaving the effect the same whether the ball be elastic or not.

to that of the striking body, is nearly equal to one third of the weight which would break the beam by pressure.

This is shown to be the case in Corollary to Problem 4; and in the impacts upon bodies suspended by wires (see remarks after our experiments upon them further on.) the maximum results differ only from the conclusions of that Problem in giving the weight of the body struck a little higher. In this 7th Conclusion, the weight of the striking body is assumed to be less than one third of what would break the beam by pressure.

Other conclusions will be deduced from the problems and theorems which follow, and which are introduced, mostly, to compare their results with those of experiment.

Prob. 1. If a ball, or other body, be suspended like a pendulum, and made to strike horizontally, at its lowest point, against the middle of a beam, A B, supported at the ends: to find the quantity of recoil of the ball, and the time of straightening of the beam.




We shall here suppose, as mentioned before, that the beam and ball have moved together as one mass from the time of the first contact to that of separation, when the beam has recovered its original form A C B.

Let C D, the whole deflection of the beam caused by the impact, $= b$, any distance D E in the recoil $= x$, the corresponding velocity $= v$, the time $= t$, the inertia of the beam $= r$, the weight of the ball $= w$, the chord of the arc ascended by the ball $= c$, the radius or length of the pendulum $= l$, the force of gravity $= g$. And let $p$ be a pressure which, applied in the middle of the beam, would bend it through a distance $e$.

Then $\dfrac{p\,(b - x)}{e}$ = pressure of the beam at E.

And since $w + r$ is the mass moved,

$$\frac{g\,p\,(b - x)}{e\,(w + r)} = \text{the accelerating force.}$$

But by mechanics,

$$v\,\frac{d\,v}{d\,x} = \frac{g\,p\,(b - x)}{e\,(w + r)}.$$

Integrating,

$$\frac{v^2}{2} = \frac{g\,p\left(b\,x - \dfrac{x^2}{2}\right)}{e\,(w + r)}$$

when $x = b$,

$$v^2 = \frac{g\,p\,b^2}{e\,(w + r)}, \qquad \therefore\ v = b\sqrt{\frac{g\,p}{e\,(w + r)}} = \text{the}$$

greatest velocity of recoil of the ball.

To find the value of the chord $c$. We have, from the nature of the circle, $\frac{c^2}{2\,l}$ = the versed sine, or height ascended. Whence the velocity due to that height is $c\sqrt{\frac{g}{l}}$.

Putting this for $v$ gives

$$c\sqrt{\frac{g}{l}} = b\sqrt{\frac{g\,p}{e\,(w + r)}}.$$

Whence

$$c = b\sqrt{\frac{p\,l}{e\,(w + r)}} = \text{the chord of the arc}$$

through which the ball would recoil if the beam were perfectly elastic.

To find the time of straightening the beam. Since, from above,

$$\frac{v^2}{2} = \frac{g\,p\left(b\,x - \frac{x^2}{2}\right)}{e\,(w + r)};$$

$$\therefore\ v = \sqrt{\frac{g\,p}{e\,(w + r)}}\ .\ \sqrt{(2\,b\,x - x^2)}.$$

And since $d\,t = \frac{d\,x}{v}$, we have

$$d\,t = \sqrt{\frac{e\,(w + r)}{g\,p}}\ .\ \frac{d\,x}{\sqrt{(2\,b\,x - x^2)}};$$

whence

$$t = \sqrt{\frac{e\,(w + r)}{g\,p}}\ .\ \text{arc}\left(\text{ver sin} = \frac{x}{b}\right).$$

And when $x = b$,

$$t = \sqrt{\frac{e\,(w + r)}{g\,p}} \times 1{\cdot}57079.$$

The time is therefore constant, when $w + r$ and $\frac{e}{p}$ are constant, whatever the deflection may be; which is analogous to the case of a vibrating chord.

Problem 2. To find the deflection of the beam answering to a given horizontal impact, and the time of its duration.

Put $c'$ = the chord of the arc through which the ball fell, $v'$ = the velocity of impact, $h$ = the height fallen through, and the rest as before.

Since from the last problem, $b\sqrt{\frac{g\,p}{e\,(w+r)}}$ = the velocity which the beam would generate, in the ball and itself, while recovering its form, this quantity will represent the velocity destroyed in both in bending the beam through the same distance $b$. Multiplying, therefore, the above by $w + r$ gives

$$b\,(w+r)\sqrt{\frac{g\,p}{e\,(w+r)}} = \text{the momentum destroyed.}$$

But $v'w$ = the momentum of the impact. And since these are equal,

$$\therefore v'w = b\,(w+r)\sqrt{\frac{g\,p}{e\,(w+r)}};$$

whence

$$b = v'w\sqrt{\frac{e}{g\,p\,(w+r)}}.$$

But $v' = \sqrt{2\,g\,h}$, from the properties of falling bodies.

$$\therefore b = w\sqrt{\frac{2\,h\,e}{p\,(w+r)}}.$$

And since $h = \frac{c'^2}{2\,l}$

$$b = w\,c'\sqrt{\frac{e}{p\,l\,(w+r)}}.$$

From the three last equations we have the deflection in terms of the velocity of the impact, the height, and the chord of the arc fallen through, the beam and ball being the same.

For the time, or the duration of an impact.—The time of deflecting the beam, through any distance $b$, must be equal to the time of returning through the same distance; the beam being supposed perfectly elastic. And since the time of return was found, by the last problem, to be $1{\cdot}57079 \times \sqrt{\frac{e\,(w+r)}{g\,p}}$, therefore twice this quantity or $3{\cdot}14159 \times \sqrt{\frac{e\,(w+r)}{g\,p}}$ = the time of an impact, or complete vibration of the beam with the striking body accompanying it. The time is therefore constant

whether the impact be great or small, provided the beam and striking body are the same, since then $\frac{e}{p}$ and $w + r$ are constant. It is, moreover, inversely as the square root of the stiffness of the beam, since $\frac{p}{e}$ measures that quality.

Cor. 1. Since, from above,

$$b = w \sqrt{\frac{2 h e}{p (w + r)}},$$

$$\therefore h = \frac{p b^2 (w + r)}{2 e w^2}.$$

If $e =$ the utmost deflection the beam will bear, and $b = e$,

$$h = \frac{p e (w + r)}{2 w^2},$$

the greatest height of impact the beam will bear from a given weight $w$.

If the weight of the beam be small and neglected, $r = o$, and

$$h = \frac{p e}{2 w}.^*$$

The two last values of $h$ being the measures of the power of a heavy and a light beam to resist impact, we have this proportion :—the power of a heavy beam : the power of a light one

$$:: \frac{p e (w + r)}{2 w^2} : \frac{p e}{2 w} :: w + r : w.$$

Whence it appears that in beams, whose strength and flexibility are the same, the power of bearing impacts from a given body may be increased to any extent by augmenting the weight of the beams. This, however, can only apply to horizontal impacts, otherwise the weight of the beam itself might break it without any blow.

* Dr. Young, speaking of the results of impact upon elastic bodies (*Natural Philosophy*, vol. i. p. 143), says, "It follows from the nature of resilience that a body of a pound weight falling from the height of a yard will produce the same effect in breaking any substance as a body of three pounds falling from the height of a foot." This it appears from above is only correct when the body struck is without weight, and the impact is given horizontally. And when the Doctor (at p. 148 and elsewhere) represents the resilience of a beam, or its resistance to impact, to be simply proportional to the bulk or weight of the beam, it was necessary to consider that the striking body was without inertia or weight.

Cor. 2. Since

$$b = w \sqrt{\frac{2 h e}{p (w + r)}},$$

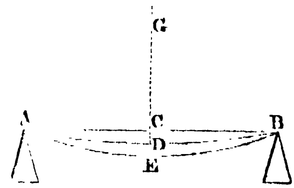
$$\therefore r = \frac{2 h e w^2}{p b^2} - w.$$

Whence we may obtain the inertia of beams, by substituting for these values in the results of our experiments. Taking them, then, from each of the seven experiments upon the third beam, these being the most varied, we have :

| | | |
|---|---|---|
| From the $8\frac{1}{2}$ lbs. balls . . . . | $r = 7·86$ lbs. | Mean 6·46 lbs. |
| From the $4\frac{1}{4}$ lbs. . . . . . . . . | $r = 5·13$ lbs. | |
| From the 9 oz. 7 drs. . . . . . . | $r = 6·39$ lbs. | |

The weight of the beam between the supports was 13·75 lbs. Whence $\frac{6·46}{13·75} = ·47$, the coefficient by which to multiply the weight between the supports of an uniform beam to obtain a weight equal to its inertia. The inertia appears therefore to differ but little from half the weight of the beam between the supports; and it will be assumed as just half in all our calculations for comparison with experiment; and especially as half the weight is the pressure which a prop under the middle, or any other part, of an uniform beam would sustain through the weight of the beam, were it cut in two in that part. Mr. Tredgold assumes it as half without proof, and calculates the effect of vertical impact as if it were horizontal.

Prob. 3. Required the height from which a body must fall upon the middle of a given beam, supported at the ends, to deflect it through an assigned distance; the weight of the beam being considered, and the striking body assumed as inelastic.




Put $e =$ C E the assigned deflection of the beam; $p =$ the pressure which, applied in the middle, would have produced it; $e' =$ C D the deflection of the beam from its own weight, $x =$ any other deflection, $q =$ the pressure in the middle of the beam from its weight ($=$ half the weight of the beam if uniform), $h =$ G D the height fallen through before impact, $n =$ the power of the deflection to which the pressure of the beam is proportional, and the rest as before.

Then, since $e^n : x^n :: p : \frac{p x^n}{e^n}$, the resistance of the beam at any

deflection $x$ would be $\frac{p\,x^n}{e^n}$, and its pressure upwards would be $\frac{p\,x^n}{e^n} - (w + q)$; and the inertia being $w + r$,

$$\therefore \text{retarding force} = \frac{g}{w + r}\left\{\frac{p\,x^n}{e^n} - (w + q)\right\}.$$

But by the laws of motion,

$$v\,\frac{d\,v}{d\,x} = -\frac{g}{w + r}\left\{\frac{p\,x^n}{e^n} - (w + q)\right\}.$$

Integrating,

$$C - \frac{v^2}{2} = \frac{g}{w + r}\left\{\frac{p\,x^{n+1}}{(n + 1)\,e^n} - (w + q)\,x\right\}.$$

The body $w$ will, by falling through $h$, have acquired a velocity $\sqrt{2\,g\,h}$; and after impact on the mass, whose inertia is $r$, they will commence going together with the diminished velocity $\frac{w}{w + r}\sqrt{2\,g\,h}$. Therefore, when $x = e'$, $v = \frac{w}{w + r}\sqrt{2\,g\,h}$, and these substituted give

$$C = \frac{w^2\,g\,h}{(w + r)^2} + \frac{g}{w + r}\left\{\frac{p\,e'^{n+1}}{(n + 1)\,e^n} - (w + q)\,e'\right\};$$

substituting for C, in the general equation, the value just obtained gives

$$\frac{w^2\,g\,h}{(w + r)^2} - \frac{v^2}{2} = \frac{g}{w + r}\left\{\frac{p\,(x^{n+1} - e'^{n+1})}{(n + 1)\,e^n} - (w + q)\,(x - e')\right\}.$$

But when the deflection is completed $v = o$, and $x = e$; and at that time, dividing by the coefficient of $h$, we obtain

$$h = \frac{w + r}{w^2}\left\{\frac{p\,(e^{n+1} - e'^{n+1})}{(n + 1)\,e^n} - (w + q)\,(e - e')\right\}. \quad . \quad . \quad (A.)$$

If the flexure of the beam is not so great as to injure its elasticity, $n = 1$, and

$$h = \frac{(w + r)\,(e - e')}{w^2}\left\{\frac{p}{2\,e}(e + e') - (w + q)\right\}. \quad . \quad . \quad . \quad (B.)$$

Or since $q$ is the pressure which accompanies the deflection $e'$, we may substitute for $q$ in terms of $e'$, by supposing that

$e : p : : e' : q$*, for then $q = \frac{p\,e'}{e}$. Putting this for $q$ gives

$$h = \frac{(w + r)(e - e')}{w^2}\left\{\frac{p}{2\,e}(e - e') - w\right\}. \quad . \quad . \quad . \quad . \quad . \quad . \qquad \text{(C.)}$$

where $e - e'$ is the deflection from impact, and $\frac{p}{e}$ is constant in impacts upon the same beam.

To obtain the value of $h$ in terms of the weights, instead of the deflections; substituting for $e - e'$ its value, $\frac{e\,(p - q)}{p}$, we obtain

$$h = \frac{e}{2\,p\,w^2}(w + r)(p - q)(p - q - 2\,w). \quad . \quad . \quad . \quad . \quad . \qquad \text{(D.)}$$

When the beam is uniform, $r = q$ nearly, as appears from Cor. 2. Prob. 2.

If the beam be very light, $q = o$, $r = o$, and

$$h = e\left(\frac{p}{2\,w} - 1\right).$$

Prob. 4. To find the weight of that beam which will bear the greatest impact from a given body falling upon it, the strength and flexibility of the beams being the same.

From the last problem

$$h = \frac{e}{2\,p\,w^2}(w + r)(p - q)(p - q - 2\,w);$$

and the question is to find $q$ when $h$ is a maximum.

In beams of the same form the inertia will bear a constant ratio to the weight; and, therefore, in similar beams, we may substitute for $r$, in terms of $q$, in the preceding formula. Suppose $a\,q = r$, then

$$h = \frac{e}{2\,p\,w^2}(w + a\,q)(p - q)(p - q - 2\,w) = \text{maximum}.$$

Neglecting the constant multiplier, and calling the other part $y$, we have

$$y = (w + a\,q)(p - q)(p - q - 2\,w) = \text{maximum}.$$

* This assumption is not strictly true: the deflection from the weight of the beam is a little greater than that due to a weight $= q$; but the error is no greater than in the supposition that the deflection $e$ in elastic beams is always as $p$, which is only the case in horizontal pressures.

Involving, and arranging according to the powers of $q$, gives

$$y = a\,q^3 - (2\,a\,p - 2\,a\,w - w)\,q^2 - (2\,p\,w \times \overline{1 + a} - a\,p^2 - 2\,w^2)\,q + w\,p^2 - 2\,w^2\,p = \text{maximum}.$$

$$\therefore \frac{d\,y}{d\,q} = 3\,a\,q^2 - 2\,(2\,a\,p - 2\,a\,w - w)\,q - (2\,p\,w \times \overline{1 + a} - a\,p^2 - 2\,w^2) = 0;$$

$$\frac{d^2\,y}{d\,q^2} = 6\,a\,q - 2\,(2\,a\,p - 2\,a\,w - w).$$

Since $\frac{d\,y}{d\,q} = 0$,

$$\therefore 3\,a\,q^2 - 2(2\,a\,p - 2\,a\,w - w)\,q = 2\,p\,w \times \overline{1 + a} - a\,p^2 - 2\,w^2.$$

From this quadratic equation we obtain

$$\left.\begin{aligned} q = \frac{2}{3}\,p - \frac{w}{3}\left(2 + \frac{1}{a}\right) \\ \mp \frac{1}{3}\sqrt{\left\{p^2 - 2\,p\,w\left(1 - \frac{1}{a}\right) + w^2\left(4 - \frac{2}{a} + \frac{1}{a^2}\right)\right\}} \end{aligned}\right\} \;.\;.\;\text{(A.)}$$

If we substitute this value of $q$ in that of $\frac{d^2\,y}{d\,q^2}$, we have

$$\frac{d^2\,y}{d\,q^2} = \mp\,2\,a\sqrt{\left\{p^2 - 2\,p\,w\left(1 - \frac{1}{a}\right) + w^2\left(4 - \frac{2}{a} + \frac{1}{a^2}\right)\right\}}.$$

The negative and affirmative signs preceding the radical here, show that in the two values of which $q$ admits, the positive radical answers to a minimum, and the negative to the maximum required.

If the beam be uniform, $r = q$ (Cor. 2. Prob. 2.), and $a = 1$. In that case, the maximum impact gives, by equation (A),

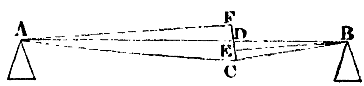
$$q = \frac{2}{3}\,p - w - \frac{1}{3}\sqrt{p^2 + 3\,w^2}. \quad .\;.\;.\quad \text{(B.)}$$

Cor. If $w$ be very small compared with $p$, then $3\,w^2$ is so compared with $p^2$. Neglecting $3\,w^2$, in the last equation, gives the value of $q$, when the resistance to impact is a maximum, $= \frac{1}{3}\,p - w$, nearly; or $q + w = \frac{1}{3}\,p$, nearly.

This corollary, and especially the more general values of $q$ in the problem, would enable us to adjust the weight of a bridge

of beams, so as best to resist concussions from a given load passing, upon an uneven road, over it. The formulæ would apply too to impacts upon some other elastic structures, $q$ being the pressure from the weight of the structure, or its inertia, in the point of impact.

Theorem. The locus of ultimate curvature of all the points in a slightly flexible beam, whose depth is equal throughout, is a parabola.




Let A D B be the natural form of the beam, supported at its ends A, B; A C B its form when bent at C to the extent of its elastic force; C F perpendicular to the curve at C; and A F, B E, perpendiculars from A B upon C F.

Then, since it is shown by writers on mechanics that the ultimate deflection of a beam is as the curvature multiplied by the square of the length, we have

C F (= deflection of the part A C) as $(\text{A C})^2 \times$ curvature,

C E (= deflection of the part B C) as $(\text{B C})^2 \times$ curvature.

But a beam of uniform depth will bear the same curvature in every part. We have therefore C F to $(\text{A C})^2$, and C E to $(\text{B C})^2$, in a constant ratio.

Putting then $l =$ A C B the length of the beam, $x =$ A C, and $c =$ a constant quantity such that C F $= c\,(\text{A C})^2$, C E $= c\,(\text{B C})^2$, we have C F $= c x^2$, C E $= c\,(l - x)^2 = c\,(l^2 - 2\,l\,x + x^2)$. Whence E F = C F − C E $= c\,l\,(2\,x - l)$.

But the right-angled triangles A F D, B E D are similar,

$$\therefore \text{B E} : \text{D E} :: \text{A F} : \text{F D};$$

and
$$\text{A F} : \text{F D} :: \text{A F} : \text{F D},$$

$$\therefore \text{A F} + \text{B E} : \text{F D} + \text{D E} :: \text{A F} : \text{F D}.$$

Here F D + D E = E F $= c\,l\,(2\,x - l)$; and as the deflection is small, A F + B E = A C B $= l$ nearly. In this case A F $= x$, B E $= l - x$, and the last proportion above becomes

$$l : c\,l(2\,x - l) :: x : \text{F D}. \quad \text{Whence F D} = c\,x\,(2\,x - l).$$

If we subtract the value of F D, just found, from that of C F $= c\,x^2$, we obtain C D $= c\,x^2 - c\,x\,(2\,x - l) = c\,x\,(l - x)$; where C D may be taken for the deflection of C, it being nearly perpendicular to A D B. Whence it appears that the ultimate deflection at any point C is as the rectangle of the segments into which the beam is divided at that point. The locus of these points is therefore a parabola.

In the preceding theorem we have supposed the ultimate cur-

vature to be the utmost the beam would bear without impairing its elasticity; but the result would have been the same if we had assumed any other constant curvature. In the parabola so formed, the deflection in the middle would be to the deflection half-way between that and one end, as 1 to ·75; and it will be seen from the mean between the results of our experiments on the 5th, 6th, and 7th beams that this ratio was found to be 1 to ·694, differing $\frac{1}{13}$ from the above.

*Theorem.*—If an uniform beam be supported at the ends, and struck horizontally upon the side, the same blow will be required to break it wherever it is given.

From Cor. 1, Prob. 2, it appears that where the striking body and the inertia of the beam remain invariable, the power of resisting impact is as the strength of the beam multiplied by the deflection it is capable of.

From the last theorem the ultimate deflection at any point is as the product of the segments at that point, and is equal to $c\,x\,(l-x)$. It is shown too by writers on the strength of materials that the strain from a weight at such point is in the same proportion; the strength therefore is inversely as the strain, or as $\frac{1}{x\,(l-x)}$. Hence the power of the beam to sustain an impact at any point is as the product of these,

$$= c\,x\,(l-x) \times \frac{1}{x\,(l-x)} = c.$$

It is therefore the same in every part.

For another proof of this see the experiments on the 5th, 6th, and 7th beams.

*Experiments.—Horizontal Impacts.*

In all the following experiments a ball was suspended by a thin string from an elevated object, and so placed that, when hanging vertical, its side just touched the beam in the point intended to be struck, which was always the *middle*, except otherwise mentioned; the beam being loosely supported at its ends, horizontally and vertically, by immoveable bodies. The impacts were made by drawing back the ball through given arcs, as measured by their chords, and letting it fall against the beam. The deflection of the beam by an impact was measured by the quantity which a peg, whose end touched the beam when at rest, had been driven into a mass of clay placed on the opposite side of the beam to that where the ball was. The use of the peg and clay was only to indicate the deflection.

In all the experiments the radius or length of the pendulum was 12 feet, except another radius be mentioned; and the magnitudes of the impacts were measured by the chords of the arcs through which the ball fell, because the velocity of impact is in that ratio, as will easily be seen; for the velocity is as the square root of the versed sine, (or of the height fallen through,) and the chord is in the same ratio.

1st Beam.—This was a rectangular bar of cast iron, 1 inch by $\frac{1}{2}$ inch section, and 4 feet 6 inches long, weighing $7\frac{1}{4}$ lbs. It was placed horizontal, and laid with its broader side against two vertical supports 4 feet asunder. The blows were given horizontally in the middle of the beam to bend it in that direction. The impinging body was a cast iron ball $8\frac{1}{2}$ lbs. weight.

| Chord of arc fallen through, in feet (Radius 12 feet.) | Observed chords of recoil of ball, in inches. | Calculated chords of recoil of ball, in inches. | Observed deflections of beam, in inches. | Calculated deflections of beam, in inches. | Difference between observed and calculated recoils. | Difference between observed and calculated deflections. |
|---|---|---|---|---|---|---|
| 1 | 7 | 8·3 | ·25 | ·26 | | |
| 2 | 15 | 14·5 | ·44 | ·53 | | |
| 3 | 21 | 21·8 | ·66 | ·79 | | |
| 4 | 31 | 29·1 | ·88 | 1·05 | | |
| 5 | 39 | 38·3 | 1·16 | 1·32 | | |
| 6 | 46 | 47·2 | 1·43 | 1·58 | | |
| | | | 1·62 | 1·84 | | |
| | Broke it. | | | | | |

Prior to the experiments above, the beam was laid flat on two horizontal supports 4 feet asunder; and weights, suspended from the middle and gently laid on, produced deflections in the bar (in the direction that it was bent by impact) as below:

28 lbs. bent it ·32 inch,

56 ———— ·63 — {Unloaded, no set.

The quantity of the recoil in the preceding experiments was calculated from the formula $r = h\sqrt{\frac{p\,l}{e\,(w + r)}}$. (Prob. 1.)

This formula, like all the others, is calculated on a supposition that the beam is perfectly elastic and the striking body devoid of elasticity.

In Cor. 2, Prob. 2, it is shown that the inertia of uniform beams, struck in the middle, is nearly equal to one half of the weight of each between its points of support; and as this beam was 4 feet 6 inches long, and 7·25 lbs. weight, the weight of 4 feet was 6·44 lbs. Whence the inertia $r = 3·22$ lbs. We have

likewise $w$ = 8·5 lbs., $l$ = 144 inches, $b$ = the different deflections; and as 56 lbs. bent the beam ·63 inch, taking $p$ = 56 lbs. we have $e$ = ·63 inch.

The quantity of the deflections in the experiments above was calculated from the formula $b = w\, c' \sqrt{\frac{e}{p\, l\, (w + r)}}$. (Prob. 2.) In this formula $c'$ is the chord of the arc the striking body fell through in each instance, and the rest are as above described.

2nd Beam.—This casting was of the same metal and dimensions as the last, and the experiment was made in the same manner, except that striking balls of different metals and weight were used. Weight of casting, (4 feet 6 inches long,) 7 lbs. Distance of supports, 4 feet, as before.

| Impact with leaden ball $8\frac{1}{2}$ lbs. weight. | | | | | Impact with cast iron ball $8\frac{1}{2}$ lbs weight. | | | | |
|---|---|---|---|---|---|---|---|---|---|
| Chord of arc fallen through, in feet. | Observed chord of recoil of ball in inches. | Calculated chord of recoil of ball, in inches. | Observed deflection of beam, in inches. | Calculated deflection of beam, in inches. | Chord of arc fallen through, in feet. | Observed chord of recoil of ball, in inches. | Calculated chord of recoil of ball, in inches. | Observed deflection of beam, in inches. | Calculated deflection of beam, in inches. |
| 1 | 6·5 | 7·3 | ·24 | ·29 | 1 | 6·5 | 7 | ·23 | ·29 |
| 2 | 13 | 14 | ·46 | ·57 | 2 | 14 | 14 | ·46 | ·57 |
| 3 | 19 | 22 | ·73 | ·86 | 3 | 20 | 19·9 | ·65 | ·86 |
| 4 | 27 | 29·7 | ·97 | 1·14 | 4 | 29 | 30 | ·98 | 1·14 |
| 5 | 34 | 39·8 | 1·30 | 1·43 | 5 | 37 | 40·4 | 1·32 | 1·43 |
| 6 | 47 | 49 | 1·60 | 1·72 | 6 | 48 | 50·5 | 1·65 | 1·72 |

| Impact with leaden ball $4\frac{1}{4}$ lbs. weight. | | | | | Impact with bell metal ball $4\frac{1}{4}$ lbs. weight. | | | | |
|---|---|---|---|---|---|---|---|---|---|
| Chord of arc fallen through. | Observed recoil. | Calculated recoil. | Observed deflection. | Calculated deflection. | Chord of arc fallen through. | Observed recoil. | Calculated recoil. | Observed deflection. | Calculated deflection. |
| 1 | 5 | 4·2 | ·11 | ·18 | 1 | 4 | 5 | ·13 | ·18 |
| 2 | 7 | 10·7 | ·28 | ·36 | 2 | 9·5 | 11·5 | ·30 | ·36 |
| 3 | 17 | 17·6 | ·46 | ·54 | 3 | 14 | 17·3 | ·45 | ·54 |
| 4 | 22 | 24·6 | ·64 | ·72 | 4 | 19 | 25 | ·65 | ·72 |
| 5 | 28 | 31·9 | ·83 | ·90 | 5 | 29 | 30·7 | ·80 | ·90 |
| 6 | 33 | 38·4 | 1·00 | 1·08 | 6 | 35 | 38·4 | 1·00 | 1·08 |

Before the experiments on impact were made upon this beam it was laid horizontal on two supports 4 feet asunder, and weights

gently laid on the middle bent it (in the same direction that it was afterwards bent by impact), as below:

28 lbs. bent it ·37 inch,
56 lbs. —— ·77 inch. Elasticity a little injured.

In calculating the deflections of the beam and the recoils of the striking body, the inertia of the beam was in this case and in all others taken as half the weight of the beam between the supports (Cor. 2, Prob. 2). Adopting the formulæ used in the calculations for the first beam, we have here the inertia $r=3{\cdot}111$ lbs. And since 28 lbs. bent this beam ·37 inch, calling $p = 28$, we have $e = {\cdot}37$. We have also $l = 144$ inches, $w = 8\frac{1}{2}$ or $4\frac{1}{4}$ lbs., and the other quantities given in each case, or obtained from the *observed* results.

The deficiency of the deflections in the experimental results, compared with the calculated ones, may perhaps arise from the resistance of the peg in the clay.

3rd Beam.—This was a rectangular bar of cast steel, 1·46 inch by ·41 inch section, and 6 feet 9 inches long, weighing $14\frac{1}{4}$ lbs. It was laid horizontally, and placed with its broader side against two vertical supports, 6 feet 6 inches asunder; the impacts being intended to bend it in its least direction. The striking bodies were balls of lead, cast iron, bell metal, and hardened steel, and of various weights, as below.

| Leaden ball $8\frac{1}{2}$ lbs. weight. | | | | | Cast iron ball $8\frac{1}{2}$ lbs. weight. | | | | |
|---|---|---|---|---|---|---|---|---|---|
| Chord of arc fallen through, in feet. (Rad. 12 feet.) | Observed chord of arc of recoil, in inches. | Calculated chord of arc of recoil, in inches. | Observed deflection, in inches. | Calculated deflection, in inches. | Chord of arc fallen through, in feet. (Rad. 12 feet.) | Observed chord of arc of recoil. | Calculated chord of arc of recoil. | Observed deflection. | Calculated deflection. |
| 1 | 5·5 | 6·37 | ·42 | ·437 | 1 | 6 | 6·37 | ·42 | ·437 |
| 2 | 11·5 | 12·4 | ·82 | ·87 | 2 | 12·5 | 13·3 | ·88 | ·87 |
| 3 | 18·3 | 18·7 | 1·23 | 1·31 | 3 | 18·0 | 19·1 | 1·26 | 1·31 |
| 4 | 24·7 | 25·2 | 1·66 | 1·75 | 4 | 24·5 | 25·6 | 1·69 | 1·75 |
| 5 | 31·0 | 32·2 | 2·12 | 2·19 | 5 | 31·5 | 32·0 | 2·11 | 2·19 |
| Leaden ball $4\frac{1}{4}$ lbs. weight. | | | | | Bell metal ball $4\frac{1}{4}$ lbs. weight. | | | | |
| 1 | | 5·17 | ·29 | ·27 | 1 | 4·5 | 5·5 | ·31 | ·27 |
| 2 | 8·5 | 10·7 | ·60 | ·54 | 2 | 9·0 | 11·0 | ·62 | ·54 |
| 4 | 17·3 | 20·0 | 1·12 | 1·08 | 4 | 17·5 | 20·0 | 1·12 | 1·08 |
| 6 | 29·0 | 30·8 | 1·73 | 1·63 | 6 | 27·0 | 29·4 | 1·65 | 1·63 |

| | | Leaden ball, weight 9 oz. 7 dr. | | | Bell metal ball, weight 9 oz. 7 drs. | | | Sheer steel ball, weight 9 oz. 7 drs. | | |
|---|---|---|---|---|---|---|---|---|---|---|
| Chord of arc each ball fell through. (Radius 3 feet.) | Calculated deflection by each ball. | Observed deflection. | Observed recoil. | Calculated recoil. | Observed deflection. | Observed recoil. | Calculated recoil. | Observed deflection. | Observed recoil. | Calculated recoil. |
| 1 | ·087 | ·07 | | ·76 | ·08 | ·8 | ·87 | ·09 | 1·0 | ·98 |
| 2 | ·17 | ·16 | 1·5 | 1·70 | ·17 | 2·0 | 1·85 | ·18 | | 1·96 |
| 3 | ·26 | ·28 | 3·5 | 3·05 | ·26 | 3·5 | 2·83 | ·27 | 3·7 | 2·94 |
| 4 | ·35 | ·45 | 4·3 | 4·90 | ·41 | 4·0 | 4·46 | ·40 | 5·0 | 4·35 |

The anomalous character of the results of the experiments with the small balls, compared with those from the larger ones, may perhaps arise from the difficulty of observing the results of small impacts.

Previous to any of the experiments on impact upon this beam, it was laid on supports 6 feet 6 inches asunder, and bent by weights in the middle, in the same direction that it was afterwards bent by impact. The results are as below:

| | |
|---|---|
| Its own weight (or pressure from half of its weight between the supports) = 6 lbs. 14 oz. | ·33 in. |
| 28 lbs. + 6 lbs. 14 oz. (its own pressure) = 34 lbs. 14 oz. .................................. | 1·19 — |
| 56 lbs. + 6 lbs. 14 oz. = 62 lbs. 14 oz. = 1006 oz. | 2·56 — |

To calculate, as before, the deflections and recoils, we had here $p = 1006$ oz., $c = 2·56$ ins., $r = 6$ lbs. 14 oz. $= 110$ oz., $w =$ the various striking weights, and the rest from the data, as in the preceding cases.

In the experiments upon this beam which were made the last of those where the impact was horizontal, additional care was taken to prevent the resistance of the clay from rendering the deflections less than they ought to be, as it was conceived that this cause had in some degree reduced the deflections in most of the other beams which had been struck horizontally.

4th Beam.—This cast iron bar was, in section, 1·08 × 1·05 inches, and 7 feet long, weighing $23\frac{1}{2}$ lbs.; it was placed against two supports, 6 feet 6 inches asunder, and bent by impacts in the middle. Weight of striking ball, of cast iron, $20\frac{3}{4}$ lbs. Radius 16 feet.

In the first of the following experiments the beam alone was struck, as before, and, in the second experiment, a 56-lbs. weight of cast iron was suspended like a pendulum and made to touch the middle of the beam; the ball was then made to impinge against the weight, and thence deflect the beam.

| Impact upon the beam to deflect it. | | | | Impact upon the weight to deflect the beam. | | | |
|---|---|---|---|---|---|---|---|
| Chord of arc fallen through, in feet. | Observed deflection, in inches. | Calculated deflection, in inches. | Difference between observed and calculated deflections. | Chord of arc fallen through. | Observed deflections. | Calculated deflections. | Difference between observed and calculated deflections. |
| 2 | ·46 | ·51 | $\frac{1}{10}$ | 2 | ·31 | ·31 | 0 |
| 3 | ·62 | ·76 | $\frac{1}{5}$ | 3 | ·43 | ·46 | $+\frac{1}{15}$ |
| 4 | ·87 | 1·01 | $\frac{1}{7}$ | 4 | ·69 | ·61 | $-\frac{1}{8}$ |
| 5 | 1·03 | 1·26 | $\frac{1}{4}$ | 5 | ·81 | ·76 | $-\frac{1}{16}$ |
| 6 | 1·24 | 1·52 | $\frac{1}{5}$ | 6 | 1·04 | ·92 | $-\frac{1}{8}$ |
| 7 | 1·44 | 1·77 | $\frac{1}{5}$ | 7 | 1·28 | 1·06 | $-\frac{1}{6}$ |
| 8 | 1·80 | 2·03 | $\frac{1}{8}$ | 8 | 1·41 | 1·22 | $-\frac{1}{7}$ |
| | | | | 9 | 1·63 | 1·36 | $-\frac{1}{6}$ |

The observed deflections in this latter case, where the impact was upon the weight, were somewhat higher than the calculated ones, while in impacts upon the beam itself they were less than as calculated. It is evident that the elasticity from the collision had here some effect, as might be expected.

Before the experiments on impact the beam was laid on two supports, 6 feet 6 inches asunder, and was bent ·78 inch by 123 lbs. (including the pressure from its own weight) applied gently in the middle.

5th, 6th, and 7th Beams.—These were three cast iron bars from the same metal, each 4 feet 6 inches long, full inch square, and 14 lbs. 10 oz. weight, nearly. They were supported by props 4 feet asunder, and the impacts were given against the beams alone; first in the middle, and afterwards half-way between the middle and one support.

The striking body was a ball of cast iron, 44 lbs. weight, suspended by a 16-feet radius.

*Impacts in the middle of the Bars.*

| Chords of arcs fallen through, in feet. | Velocities per second, computed from the chords | Deflections of the 5th beam, in parts of an inch. | Deflections of the 6th beam, in parts of an inch. | Deflections of the 7th beam, in parts of an inch. | Mean between the deflections of the 6th and 7th beam, in the middle. |
|---|---|---|---|---|---|
| 1 | 1·414 | ·23 | | | |
| 1·5 | 2·121 | ·28 | | ·26 | ·26 |
| 2 | 2·828 | ·42 | ·33 | ·37 | ·35 |
| 2·5 | 3·535 | ·50 | ·42 | ·49 | ·46 |
| 3 | 4·242 | ·59 | ·53 | ·56 | ·55 |
| 3·5 | 4·949 | ·67 | ·68 | ·67 | ·67 |
| 4 | 5·656 | ·82 | ·79 | ·76 | ·77 |
| 4·5 | 6·363 | | ·85 | | ·85 |
| 5 | 7·070 | ·97 | ·97 | ·92 | ·95 |
| 5·5 | 7·777 | | 1·05 | | 1·05 |
| 6 | 8·484 | Broke in the middle. | | | |

The sixth and seventh beams having borne the deflections 1·05 and ·92 as above, and the former being considered (from the result of the fifth beam) very near to fracture, they were no longer struck upon the middle, but upon a point half way between the middle and one support; the pendulous ball being so ordered as just to touch that point of the beam when hanging vertical. The intention was to ascertain the deflection of each beam in this new point of impact, and the blow necessary to break it there.

*Impacts at half the distance between middle and one support.*

| Chords of arcs fallen through, in feet. | Deflection of the 6th beam in point struck, in parts of an inch. | Deflection of the 7th beam in point struck, in parts of an inch. | Mean between deflections of the 6th and 7th beam at one fourth distance from one end. | Ratio of deflections from equal impacts at one fourth span and middle of beam. | |
|---|---|---|---|---|---|
| 2 | ·22 | ·25 | ·24 | $\frac{24}{35}$ = ·69 | |
| 3 | ·40 | ·44 | ·42 | $\frac{42}{55}$ = ·76 | |
| 4 | ·50 | ·54 | ·52 | $\frac{52}{77}$ = ·68 | mean ·694 |
| 5 | ·60 | ·68 | ·64 | $\frac{64}{95}$ = ·67 | |
| 5·5 | ·70 | | ·70 | $\frac{70}{105}$ = ·67 | |
| 6 | Broke the beam. | Broke it. | | | |

Both beams were broken exactly in the point of impact, and with the same intensity of blow, an impact through an arc of 6 feet. Now an impact, through an arc of 6 feet, against the *middle* of the 5th beam, had broken it; and as impacts in the middle through 3, 4, and 5 feet, had nearly bent each of the three beams through equal quantities, we may be convinced that an impact through about 6 feet against the middle of the 6th and 7th beams would have broken them. Hence we may conclude that an uniform beam will bear the same blow whether struck in the middle or half way between that and one end.

Previous to the preceding experiments upon the 7th beam, it was laid horizontal on two props, 4 feet asunder, and weights gently laid on the middle bent it as below:

168 lbs. bent it ·26 inch; unloaded, no set.
224 lbs. —— ·40 — ; unloaded, set $\frac{1}{20}$ inch.

Remark.—From all the preceding experiments it appears that the deflection is nearly as the chord of the arc fallen through, or as the velocity of impact.

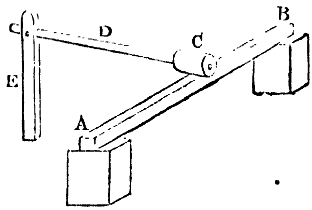
*Vertical Impacts.*

In the following experiments the impacts were given by a short leaden cylinder C falling from different small heights upon

the middle of the beam, which was sustained at its ends A, B, in a horizontal position, by firm supports. The cylinder C was perforated by one end of an uniform light slip of timber D, 6 feet 4 inches long, which was attached by a joint at the other end to the vertical prop E.

It will be observed that C fell through small arcs of a circle instead of vertical; but as the radius was large and the heights usually small, the error was not worth notice.

| | lbs. | oz. |
|---|---|---|
| Weight of leaden cylinder . . | 13 | 4 |
| $\frac{1}{3}$ of weight of lever D (its inertia) . . . . . . . . | 0 | 9 |
| Weight of striking body, or sum of the above . . . | 13 | 13 |




8th Beam.—This bar was of cast steel, and was that called the 3rd beam in our horizontal experiments; it was now laid upon two supports 6 feet 6 inches asunder as before, and bent by impact in the same direction.

| Heights fallen through, in inches. | Observed deflections of beam from impact. in inches. | Whole deflections of beam or observed deflections + ·33 inch. | Calculated pressure, in ounces, which would produce the whole deflections. | Calculated heights fallen through, in inches. | Excess of real over calculated heights fallen. |
|---|---|---|---|---|---|
| 3 | 2·00 | 2·33 | 916 | 2·53 | $\frac{1}{6}$ |
| 6 | 2·57 | 2·90 | 1140 | 5·22 | $\frac{1}{8}$ |
| 9 | 2·99 | 3·32 | 1305 | 7·76 | $\frac{1}{7}$ |
| 12 | 3·40 | 3·73 | 1466 | 10·69 | $\frac{1}{9}$ |
| 15 | 3·74 | 4·07 | 1599 | 13·45 | $\frac{1}{10}$ |
| 18 | 4·15 | 4·48 | 1760 | 17·19 | $\frac{1}{21}$ |
| 21 | 4·40 | 4·73 | 1859 | 19·71 | $\frac{1}{16}$ |
| 24 | 4·68 | 5·01 | 1969 | 22·72 | $\frac{1}{19}$ |

Referring to the previous experiments on this beam, it will be found that the beam when placed on two supports 6 feet 6 inches asunder, bent ·33 inch, as given in the 3rd column above, by its own weight; and that 1006 ounces laid on the middle bent it 2·56 inches.

The calculated heights of impact in the 5th column were obtained from the formula

$$h = \frac{e}{2\,p\,w^2}(w + r)\,(p - q)\,(p - q - 2\,w), \text{ problem 3rd.}$$

In this formula $\frac{e}{p}$ is constant in pressures upon the same

beam; and we have here $\frac{e}{p} = \frac{2\cdot56}{1006}$, $w = 13$ lbs. 13 oz. $= 221$ oz., $q = r = 110$ oz.; and $p$ is obtained from each of the tabulated pressures in the 4th column.

The values of $h$ would have been calculated more easily than as above by equation (C), prob. 3; but the formula used was conceived to stand in need of a test.

9th Beam.—This was a bar of cast steel $2\cdot05 \times \cdot52$ section, and 6 feet 4 inches long, weighing $23\frac{1}{4}$ lbs. It was laid on supports 6 feet asunder, and the leaden cylinder was let fall upon the middle as before.

This bar was bent 2·44 inches by 224 lbs. being laid gently on the middle.

| Heights fallen through, in inches. | Deflections from impact, in inches. | Calculated heights fallen through. | Excess of real over calculated heights fallen. |
|---|---|---|---|
| 6 | 1·09 | 5·14 | $\frac{1}{7}$ |
| 12 | 1·42 | 9·49 | $\frac{1}{5}$ |
| 18 | 1·76 | 15·34 | $\frac{1}{7}$ |
| 24 | 2·02 | 20·74 | $\frac{1}{7}$ |

The heights, in this case, were calculated by equation (C), problem 3.

10th Beam.—This was an uniform tube of wrought iron: it was laid horizontal on two supports, 9 feet 6 inches asunder, and struck upon the middle with the cylinder falling upon it; first, when the tube was empty, and next, when it was filled with leaden bullets: the bullets being put rather loosely in to increase its weight without affecting its flexibility.

Weight of the empty tube, between the supports, 9·2 lbs. Weight of tube, when loaded, 17·7 lbs. Deflection of empty tube, from its own weight, ·26 inch. Deflection from weight of loaded tube ·61 inch. Deflection from 28 lbs. hung in the middle of unloaded tube 1·47 inch, and of loaded tube 1·44 inch.

| Empty tube. | | | Tube loaded with bullets. | | |
|---|---|---|---|---|---|
| Heights fallen through, in inches. | Deflections from impact, in inches. | Whole deflections, in inches. | Heights fallen through, in inches. | Deflections from impact, in inches. | Whole deflections, in inches. |
| 3 | 2·50 | 2·76 | 3 | 2·31 | 2·92 |
| 6 | 3·20 | 3·46 | 6 | 2·99 | 3·60 |
| 9 | 3·74 | 4·00 | 9 | 3·44 | 4·05 |
| 12 | 4·02 | 4·28 | 12 | 3·96 | 4·57 |

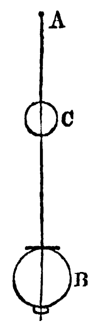
In these experiments the deflections from impact upon the

loaded tube were less than those where the empty one was used, though the ultimate deflections were a little greater in the former case than in the latter. The following experiments are intended to show that increasing, to a certain extent, the weight of the body struck augments its power of bearing impact.

### *Impact on Bodies sustained by Wires.*

### Experiments to illustrate the Increase of Power to resist Impulsion, which elastic Bodies acquire by being loaded.

An iron wire (thickness No. 17) was suspended by one end from the top of a room; the other hanging down with a round cast iron ball, having a hole bored through it, affixed to the lower end. The striking body, likewise, was a cast iron ball, bored in like manner, and having the wire passing through it so easily as to form no resistance as it fell.




In the annexed figure, A B represents the wire fastened at A; B the weight at bottom; C the striking weight. In the experiments, C was let fall upon B from various distances, C B, to ascertain the heights necessary to break the wire when sustaining different weights B.

The weight B had a perforated disc of lead, through which the wire passed, laid upon it, to lessen or destroy the elasticity of the impinging bodies.

### *Experiments.*

| Length of wire. | | Weight of striking ball at C. | | Weight of ball at B with lead. | | Bore impacts through. | Broke with falling through. | Remarks. |
|---|---|---|---|---|---|---|---|---|
| ft. | in. | lbs. | oz. | lbs. | oz. | | feet. | |
| 25 | 0 | 5 | 14 | 0 | 9 | 2, 2½, 3, 3½, 4 feet. ......... | 4½ } 4¼ | No lead. |
| — | | — | | — | | (repeated) 2½, 3, 3½, 4, 4½ ft. | 5 | |
| 24 | 0 | 6 | 0 | 10 | 1 | 7 ft.. | 7½ } 7 | The wire usually broke a few inches from the bottom, about the point of impact; and it was adjusted to its length (if fresh wire were not used) by a reserve at the top. |
| — | | — | | — | | (repeated, fresh wire,) 6 ft. | 6½ | |
| — | | — | | 44 | 0 | 1, 2, 3, 4, 5, 6, 6½, 7 ft. ... | 7½ | |
| — | | — | | 80 | 8 | 6, 6½, 7, 7½, 8, 8½, 9 ft. ... | 9½ | |
| — | | — | | 89 | 0 | 8, 8½, 9, 9½, 10, 10½ ft. ... | 11 | |
| — | | — | | 125 | 0 | 8, 8½, 9, 9½, 10 ft. ......... | 10½ | |
| — | | 40 | 0 | 10 | 1 | 3, 4 inches .................. | 5 inches. | |
| — | | — | | 80 | 8 | 2, 3, 4, 5, 6 inch............ | 7 do. | |
| — | | — | | 89 | 0 | 4, 5 inch. ..................... | 6 do. | It broke one inch from top. |
| 24 | 8 | 85 | 0 | 14 | 0 | 2 inch. ........................ | 3 do. | |

The bodies struck above were all single balls of cast iron, with the flat piece of lead laid upon them.

To ascertain the strength and extensibility of this wire, it was broken, in a very careful experiment, with $252\frac{1}{2}$ lbs. suspended at its lower end and laid gradually on. And to obtain the increment of a portion of the wire (length 24 feet 8 inches) when loaded by a certain weight, it had 139 lbs. hung at the bottom, and when 89 lbs. were taken off that load, the wire decreased in length ·39 inch.

Whence 89 lbs. : ·39 : : 252·5 lbs. : 1·12 inch = the ultimate extension of 24 feet 8 inches.

∴ 1·09 inch = ultimate extension of 24 feet, or extension of that length of the wire when loaded to its breaking point.

Remarks.—1st. Should it be suggested that the wire by being frequently impinged upon would perhaps be much weakened, the author would beg to refer to a paper of his on Chain Bridges, *Manchester Memoirs*, 2nd series, vol. v., where it is shown that an iron wire broken by pressure several times in succession is very little weakened, and will nearly bear the same weight as at first.

2nd. The first of the preceding experiments on wires are the only ones from which the maximum can, with any approach to certainty, be inferred; and we see from them that the wire resisted impulsion with the greatest effect when it was loaded at bottom with a weight which, added to that of the striking body, was a little more than one third of the weight that would break the wire by pressure, a conclusion which does not differ widely from that of Cor. to Prob. 4.

3rd. From these experiments generally, it appears that the wire was weak to bear a blow when lightly loaded.

These last experiments and remarks, and some of the preceding ones, show clearly the benefit of giving considerable weight to elastic structures subject to impact and vibration; and the weight of greatest resistance may frequently be calculated from the formulæ in problem 4 or its corollary.

# *Observations on the Direction and Intensity of the Terrestrial Magnetic Force in Ireland, made by the Rev.* HUMPHREY LLOYD, *M.A., F.R.S., &c., Professor of Natural Philosophy in the University of Dublin; by Captain* EDWARD SABINE, *R.A., F.R.S., &c.; and by Captain* JAMES CLARKE ROSS, *R.N., F.R.S., &c.*

[With a Plate.]

THE observations which form the subject of the present communication were made during the years 1834 and 1835, in compliance with the recommendation of the British Association urged in the first and second Reports of its proceedings. Their main object has been to determine the direction of the lines of magnetic dip and intensity in Ireland, and to make a small, but it was hoped exact, addition to our knowledge of the laws of distribution of the earth's magnetism. The observations are threefold: first, observations of the horizontal part of the earth's magnetic force, as determined by the time of vibration of a needle suspended horizontally, after the method of Professor Hansteen; secondly, observations of dip, made in the usual manner; and thirdly, observations of dip and intensity at the same time, and with the same instrument, according to the method adopted by Professor Lloyd, and already submitted to the Association *.

## 1. *Horizontal Intensity.*

The instruments employed in the first series of observations were constructed after the model of that of Professor Hansteen. The needles are cylinders $2\frac{1}{2}$ inches long, and ·13 of an inch in diameter, suspended by a few filaments of the silkworm's thread. They are inclosed in a small rectangular box, supported upon levelling screws, and having a tubular pillar screwed on at top for the silk suspension. At the bottom of the box is a divided circle, for the purpose of noting the arc of vibration: the temperature is observed by means of a small thermometer inclosed in such a manner as to avoid contact with the bottom and sides of the box. Before the commencement of the observations, the bottom of the box is to be rendered truly horizontal by means of the levelling screws on which it rests, and of a small spirit level with which it is furnished. The needle being then sus-

* *Fourth Report*, p. 557. *Transactions of the Royal Irish Academy*, vol. xvii.

pended so as to hang near the bottom, its deviation, if any, from the horizontal position will be detected by its inclination to the surface. It is then to be slightly moved to one side or other in the brass stirrup by which it is supported, until it hangs truly parallel to the lower surface of the box; and when this adjustment is once accurately made, no further alteration will be required, unless the change of dip be considerable.

When an observation is to be made, the needle is raised or lowered by a small roller to which the silk suspension is attached, so that it may hang about midway between the upper and lower surfaces of the box. It is then drawn aside from the magnetic meridian through an arc of 25° or 30°, by a piece of brass wire inserted in the side of the box, and is allowed to oscillate. The registry of the oscillations is commenced when the amplitude of the vibration on either side of the meridian is reduced to 20°, and it is continued during 360 vibrations; the moment of the completion of every 10th vibration during that interval being noted by a chronometer. The amplitude of the final arc, or of the arc of the 360th vibration, is also observed; and the temperature of the air in the box, as indicated by the interior thermometer, is noted at the beginning and end of the observation.

It is obvious that in this manner seven intervals of time are obtained, each corresponding to 300 vibrations,—viz. the interval between the 0th and 300dth vibration, between the 10th and 310th, &c., and between the 60th and 360th;—and the mean of these is taken as the result. But to this result several corrections must be applied.

1. The time as shown by the chronometer is to be corrected for *rate*; and accordingly the chronometer's rate must be determined from time to time by comparison with a good timekeeper, or by astronomical observations. In the present series the rate was observed at the commencement and end of each group of observations by the former and easier method. The amount of the correction due to rate is in most cases very small, the correction in the time of 100 vibrations corresponding to a daily rate of 2″ being less than 0″·01 with the slowest of the needles employed.

2. Professor Hansteen has applied a correction for the arc of vibration, so as to reduce the time to that corresponding to infinitely small arcs. The correction is investigated on the same principles as that usually applied to pendulum observations. It is however more complicated in its form; for, instead of a single series of vibrations, (as in the case of the pendulum,) we have here seven distinct series, each commencing from a different

arc. The principle, however, seems hardly applicable in the present instance. It is assumed that the successive arcs of vibration decrease in geometric progression, as they must necessarily do if the resistance of the air be proportional to the velocity. This is found to hold good in the vibrations of the pendulum when the arcs are very small; but it is by no means true when they are so considerable as those in which the horizontal magnetic pendulum is made to vibrate. Where, however, the vibrations commence from the same arc, and the terminal arc does not much vary, the correction itself may perhaps be disregarded. In the following observations, in which the initial arc was 20°, the 360th or terminal arc was generally $2\frac{1}{2}°$, and was in all cases included between the limits 1° and 4°. In such cases, then, the correction must be, nearly, a constant quantity; its application to the observed times is therefore nearly equivalent to their multiplication by a constant coefficient, and the ratio of the times (with which alone we are concerned in this class of observations) remains unaltered. For these reasons no attempt has been made to introduce a correction for the arcs in the following results; but the terminal arcs are given, so as to put the reader in possession of all the circumstances of the observation.

3. By far the most important correction is that due to temperature. If $T'$ be the observed time of 100 vibrations corresponding to the actual temperature $t'$, and $T$ the corrected time corresponding to the standard temperature $t$, the correction is

$$T - T' = a\,T'\,(t - t');$$

$a$ bring a constant coefficient whose value is to be determined experimentally for each needle.

The following observations were made with the cylinders L ($a$), L ($b$), in order to determine the value of the coefficient $a$ for each. The apparatus being inclosed in a large glass bell, the time of 100 vibrations of cylinder L ($a$), commencing with the arc of 10°, was observed at the mean temperature of the room, and when the air of the bell was heated artificially from below, by means of a spirit lamp. The final arc varied between 4° and 5°. The observations with cylinder L ($b$), were made in the bell without the apparatus. In this case no means were taken to observe with any accuracy the arc of vibration; and in order to reduce as much as possible any error arising from this source, the observations were continued in each instance until the arcs were reduced to the smallest appreciable, and the mean of the last five intervals of 100 vibrations then taken as the result. The chronometer's rate varied from + 0″·6 to + 1″·4

per diem, and had therefore no appreciable influence on the results.

CYLINDER L ($a$).

| Date. | Hour. | Time. | Temp. |
|---|---|---|---|
| | h. m. | s. | ° |
| March 4. | 1 42 | 212·64 | 51·5 |
| | 1 55 | 212·82 | 54·5 |
| —— 8. | 3 46 | 212·30 | 58·8 |
| | 4 1 | 212·21 | 58·0 |
| | Mean | 212·50 | 56·5 |
| —— 1. | 3 41 | 213·96 | 82·5 |
| | 4 10 | 213·96 | 83·1 |
| —— 8. | 2 6 | 213·91 | 74·5 |
| | Mean | 213·95 | 80·0 |

CYLINDER L ($b$).

| Date. | Hour. | Time. | Temp. |
|---|---|---|---|
| | h. m. | s. | ° |
| March 19.* | 11 7 | 293·48 | 53·6 |
| | 11 40 | 292·52 | 55·0 |
| | 2 42 | 291·00 | 60·3 |
| | 3 5 | 293·76 | 59·5 |
| | Mean | 293·44 | 57·1 |
| | 12 50 | 295·24 | 80·0 |
| | 1 16 | 295·20 | 83·3 |
| | Mean | 295·22 | 81·6 |

The constant coefficient sought is to be calculated from the formula

$$a = \frac{T - T'}{T'(t - t')},$$

in which $t$ and $t'$ are the two temperatures, and T and T′ the corresponding times of vibration. We find

Cyl. L ($a$). $T' = 212''{\cdot}50$, $T - T' = 1''{\cdot}45$, $t - t' = 23^\circ{\cdot}5$
$a = {\cdot}000254$

Cyl. L ($b$). $T' = 293''{\cdot}44$, $T - T' = 1''{\cdot}78$, $t - t' = 24^\circ{\cdot}5$
$a = {\cdot}000248$.

Stopping at the fifth decimal place, then, the coefficient for temperature for both cylinders is ·00025. It is to be observed that these cylinders were made at the same time, and were therefore probably tempered to the same degree; and to this circumstance we may, with much probability, ascribe the close agreement in the values of the constant which determines the effects of temperature upon the force of the needle.

No observations were made to determine directly the effects of temperature upon the other needles employed in the course of these observations; and, in correcting the results obtained with them, the coefficient employed by M. Hansteen, viz.,

* A series of observations had been made with this cylinder, in the same manner and on the same days as those with cyl. L ($a$); but the results were unsatisfactory, some of them indicating an *increase* of force with increased temperature. Such contradictory results have been noticed by many observers, and are usually attributed to the disturbing effects of currents of air, determined by inequality of temperature.

·00017, has been that adopted. The *standard* temperature ($t$), to which all the results contained in the following pages are reduced, is 60° Fahr.

4. All that we know of the diurnal variations of the intensity of the horizontal force, is due to M. Hansteen and Professor Christie. These writers agree in fixing the hours of minimum intensity at 10½ A.M. The intensity then increases, and attains its maximum, according to Professor Christie, at about 7½ P.M. The amount of this maximum is 1·0024 in summer, the minimum intensity being unity; but this amount, as well as the hour of its occurrence, changes with the season. Of the law according to which the force varies between its two limiting values, we know nothing; and it is therefore impossible, in the present state of our knowledge, to apply a correction for these variations. It was proposed to evade this difficulty, in the ensuing observations, by observing at a fixed hour. To this limitation, however, it was found impracticable to adhere, and the results still remain uncertain by the amount of the diurnal change.

5. The variations of the magnetic force give rise to another and still graver class of errors. The least experience in observations of this nature is enough to prove that the horizontal intensity is, from some cause or other, subject to irregular fluctuations; and these fluctuations, like those of the barometer in our climates, are much more considerable than the regular horary changes. It seems probable that these variations in the intensity of the horizontal force are, like those in its direction, not local phenomena, but occur at the same time at places widely separated. To eliminate them from our results, therefore, it would suffice to have a regular series of observations made at some fixed station, cotemporaneous with those made at the different stations; and, if these be not very remote, we may assume that the variation of the observed force at each from its mean amount is the same as that observed at the same time at the fixed station. Unhappily these means of freeing the results from the admixture of what may be called accidental phenomena have not been attended to in the following, or indeed in any similar series of observations, and there is reason to believe that the errors due to this cause are the largest in amount of any by which the present series is affected.

The amount of these fluctuations, from day to day, may be judged of from the following specimen of a series of observations such as that alluded to, commenced by Captain Sabine in the month of June 1835. The apparatus in which the needle was vibrated was unmoved during the continuance of the series, and the needle remained permanently suspended. The height of the

barometer was noted, as well as the temperature; the hour of observation was, nearly, 10 A.M.

*Time of 200 Vibrations of a Standard Needle.*

| Date. | | Barom. | Therm. | Time. |
|---|---|---|---|---|
| | | inch | ° | s. |
| June | 15. | 30·260 | 66·0 | 1139·60 |
| | 16. | 30·268 | 64·5 | 1138·20 |
| | 17. | 30·188 | 62·0 | 1138·27 |
| | 18. | 30·188 | 61·0 | 1135·87 |
| | 19. | 30·200 | 59·0 | 1136·67 |
| | 20. | 30·080 | 62·0 | 1135·93 |
| | 21. | | 62·0 | 1137·67 |
| | 22. | 29·640 | 61·0 | 1138·53 |
| | 23. | 29·682 | 59·0 | 1138·40 |
| | 24. | 29·360 | 57·5 | 1137·80 |
| | 25. | 29·850 | 57·0 | 1136·87 |
| | 26. | 29·580 | 57·0 | 1136·70 |
| | 27. | 30·165 | 57·0 | 1137·13 |
| | 29. | 30·120 | 59·0 | 1137·80 |
| | 30. | 29·820 | 58·0 | 1137·73 |
| July | 2. | 29·990 | 62·0 | 1137·30 |
| | 4. | *29·850* | *60·5* | *1138·73* |
| | 5. | *29·650* | *60·0* | *1139·53* |
| | 6. | 30·000 | 58·5 | 1136·60 |
| | 7. | 29·850 | 59·0 | 1137·87 |

The mean time of 200 vibrations, deduced from these results, is $1137''\cdot66$ at the temperature $60°\cdot6$. But the time observed on the 18th of June is $1135''\cdot87$ at $61°\cdot0$; so that on this day the rate of the needle was less than the mean by $1''\cdot79$, a difference which corresponds to an increase of ·003 in the horizontal force. The observation of the 5th of July exhibits a difference somewhat greater on the other side.

6. The last source of error which requires to be noticed under this head, is the change of the magnetic condition of the needles employed. Independently of the derangements of magnetic equilibrium induced by the presence of iron, or other disturbing causes, it is well known that most needles lose something of their original force. This loss is greatest at first; and the needle, if originally well tempered and then magnetized to saturation, is usually found to arrive at a nearly settled state in about a year. Most of the cylinders employed in the following observations seem to have reached that condition; and the changes of magnetic state which they have exhibited are, except in the case of Cyl. S (*b*), unimportant. In order to detect any such changes, and to correct for them if they arise, it is necessary to observe at

the place chosen as the base of reference, at the termination of each series of observations, as well as at their commencement. If it is then found that the needle has lost any small portion of its force, or if the time of vibration has augmented, the amount of the correction due to each result may be found by assuming the change to have been *regular*, or proportional to the time elapsed. When the loss is very small, however, (as was the case in the observations which form the subject of this paper,) the correction may be disregarded, provided we take as the time of vibration at the base of reference the mean of the times observed at the commencement and end of the series.

The needles used in the present series are the cylinders L (*a*), L (*b*), made by Dollond, and belonging to Mr. Lloyd; cylinder S (*b*) belonging to Captain Sabine, and cylinders R (*c*) and R (*d*) in the possession of Captain James Ross. All the circumstances of the observations are given in the annexed Table. The first, second, and third columns contain the *place*, *day of the month*, and *hour* of the observation. In the fourth column is set down the *observed time* of 100 vibrations, or the immediate result of observation divided by 3. The fifth column contains the *terminal arc*, the initial arc being in all cases 20°. In the sixth column is given the *chronometer's rate*; in the seventh the *temperature*; and in the eighth the deduced or *corrected time*. The hour, set down in the third column as the hour of observation, is the mean of the commencement and end; and the recorded temperature is also the mean of those observed at the beginning and end of the observation. It will appear from the preceding that the corrections employed in deducing the corrected from the observed times are those due to temperature and to the rate of the chronometer.

Beside the observations which follow there were some others of an earlier date, made for the purpose of comparing the horizontal intensity at Dublin and Limerick, the two stations with which all the other places in Ireland have been immediately compared. In the observations alluded to, the rate of cylinder S (*b*), was observed in the Philosophy School, Trinity College; and the local attraction of the building was determined by subsequent comparisons of the force there with that in the garden, Trinity College, the place which was afterwards selected for all the Dublin observations. These earlier comparisons, as well as some other imperfect ones obtained previously to the autumn of 1834 with two other cylinders, have not been included in the annexed Tables; partly because the needles employed do not seem to be as trustworthy as the rest, but chiefly because of the uncertainties of the double comparison which they involve.

## Table I.

*Time of Vibration of Cylinder* S (*b*)*.

| Place †. | Date. | Hour. | Time. | Arc. | Rate. | Temp. | Corr. Time. |
|---|---|---|---|---|---|---|---|
| | | h m | " | | " | ° | " |
| Limerick ...... | July 23. | 1 44 | 400·23 | Between 2° and 3° | 0·0 | 66·0 | 399·82 |
| | | 2 14 | 400·33 | | ... | 67·0 | 399·85 |
| | 25. | 12 20 | 401·19 | | ... | 62·5 | 401·02 |
| | | 12 50 | 400·77 | | ... | 61·5 | 400·67 |
| | Mean | ...... | 400·63 | ... | ... | 64·2 | 400·34 |
| London ...... | Aug. 20. | 11 22 | 390·14 | 3·0 | —0·3 | 65·0 | 389·81 |
| | | 11 46 | 390·08 | 3·0 | ... | 66·2 | 389·67 |
| | | 12 44 | 390·34 | 2·5 | ... | 66·0 | 389·94 |
| | 21. | 10 58 | 389·98 | 2·5 | ... | 62·7 | 389·80 |
| | | 11 29 | 389·88 | 3·0 | ... | 63·0 | 389·68 |
| | | 12 0 | 389·70 | 2·8 | ... | 62·5 | 389·53 |
| | | 12 31 | 389·78 | 3·0 | ... | 62·0 | 389·65 |
| | 22. | 10 47 | 390·18 | 2·5 | ... | 63·0 | 389·98 |
| | | 11 17 | 389·93 | 3·0 | ... | 62·5 | 389·76 |
| | | 11 47 | 389·82 | 3·0 | ... | 61·0 | 389·75 |
| | | 12 33 | 389·76 | 3·0 | ... | 61·5 | 389·66 |
| | 23. | 10 57 | 389·76 | 2·5 | ... | 60·5 | 389·73 |
| | | 11 37 | 389·78 | 3·0 | ... | 60·5 | 389·75 |
| | | 12 13 | 389·37 | 3·0 | ... | 60·5 | 389·34 |
| | | 12 45 | 389·32 | 3·0 | ... | 61·0 | 389·25 |
| | 27. | 10 27 | 389·50 | 2·8 | ... | 57·0 | 389·70 |
| | | 11 2 | 389·33 | 2·5 | ... | 57·5 | 389·50 |
| | | 11 36 | 389·30 | 2·5 | ... | 56·5 | 389·53 |
| | | 12 13 | 389·43 | 3·0 | ... | 57·0 | 389·63 |
| | | 12 49 | 389·43 | 3·0 | ... | 57·0 | 389·63 |
| | Mean | ...... | 389·74 | ... | ... | 61·2 | 389·66 |
| Limerick ...... | Sept. 9. | 12 17 | 403·70 | Between 2° and 3°. | 0·0 | 61·0 | 403·63 |
| Ballybunan ... | 16. | 5 20 | 404·29 | | ... | 62·0 | 404·15 |
| Glengariff ... | 27. | 11 20 | 402·55 | | ... | 66·0 | 402·14 |
| Killarney ...... | Oct. 4. | 11 40 | 403·93 | | ... | 65·5 | 403·56 |
| Limerick ...... | 9. | 12 43 | 404·56 | | ... | 53·5 | 405·01 |
| Tulla ........ | 12. | 8 50 | 404·16 | | ... | 52·0 | 404·70 |
| Templemore... | 17. | 8 36 | 395·82 | | | 51·0 | 396·43 |

* The observations in London made by Captain James Ross; all the others with this needle by Captain Sabine.

† Limerick, garden at Somerville, one mile from town.—London, Regent's Park.—Ballybunan, field adjoining the inn.—Glengariff, Mr. Eccles's garden. —Killarney, Mucross demesne.—Tulla, Kiltanon, Mr. Molony's garden.—Templemore, Sir H. Carden's grounds; local attraction suspected.

TABLE I.—(*Continued.*)

| Place *. | Date. | Hour. | Time. | Arc. | Rate. | Temp. | Corr. Time. |
|---|---|---|---|---|---|---|---|
| | | h m | ″ | | ″ | ° | ″ |
| Clonmel ...... | Oct. 19. | 9 0 | 402·16 | Between 2° and 3°. | 0·0 | 55·0 | 402·50 |
| Limerick ...... | 29. | 12 6 | 404·16 | | ... | 55·5 | 404·47 |
| Fermoy ...... | Dec. 2. | 3 45 | 399·93 | | ... | 48·0 | 400·75 |
| Limerick ...... | 10. | 2 45 | 402·67 | | ... | 42·0 | 403·89 |
| London ...... | July 4. | 2 43 | 402·36 | 1·0 | ... | 70·0 | 401·68 |
| | | 3 14 | 402·38 | 1·5 | ... | 71·0 | 401·63 |
| | 5. | 9 54 | 401·84 | 1·0 | ... | 62·0 | 401·70 |
| | | 10 21 | 401·73 | 1·5 | ... | 64·0 | 401·46 |
| | | 11 13 | 401·02 | 2·0 | ... | 60·0 | 401·02 |
| | | 11 46 | 401·50 | 1·5 | ... | 62·0 | 401·36 |
| | | 4 48 | 401·74 | 1·5 | ... | 64·0 | 401·47 |
| | 6. | 9 53 | 401·36 | 2·0 | ... | 65·0 | 401·02 |
| | | 10 30 | 401·96 | 2·0 | ... | 68·0 | 401·42 |
| | 7. | 9 32 | 400·96 | 2·0 | ... | 60·0 | 400·96 |
| | | 10 6 | 401·09 | 1·5 | ... | 62·0 | 400·95 |
| | | 10 43 | 401·53 | 1·5 | ... | 64·0 | 401·26 |
| | Mean | ...... | 401·62 | ... | ... | 64·3 | 401·33 |
| Limerick ...... | July 27. | 4 0 | 413·54 | ... | −12· | 76·2 | 412·41 |
| | 28. | 11 58 | 412·83 | ... | ... | 66·8 | 412·35 |
| | Mean | ...... | 413·18 | ... | ... | 71·5 | 412·38 |

*Time of Vibration of Cylinder* L (*a*)†.

| Place ‡. | Date. | Hour. | Time. | Arc. | Rate. | Temp. | Corr. Time. |
|---|---|---|---|---|---|---|---|
| | | h m | ″ | | ″ | ° | ″ |
| Limerick ...... | Sept 9. | 1 23 | 243·26 | ... | 0·0 | 60·0 | 243·26 |
| Ballybunan ... | 17. | 10 35 | 243·87 | ... | ... | 63·0 | 243·69 |
| Glengariff ... | 27. | 12 0 | 242·29 | ... | ... | 68·0 | 241·80 |
| Killarney ...... | Oct. 4. | 12 15 | 242·46 | ... | ... | 66·0 | 242·09 |
| Limerick ...... | 8. | 1 14 | 243·16 | 4·0 | +15· | 62·0 | 243·00 |
| Dublin......... | 10. | 2 31 | 243·71 | 3·0 | + 1· | 55·2 | 244·00 |
| | | 2 50 | 243·81 | 3·5 | ... | 55·0 | 244·11 |
| | 11. | 2 4 | 243·52 | 3·5 | ... | 59·0 | 243·58 |
| | Mean | ...... | 243·68 | ... | ... | 56·4 | 243·90 |
| Armagh ...... | 14. | 2 22 | 246·23 | 3·5 | ... | 49·5 | 246·88 |
| | 15. | 2 16 | 246·30 | 4·0 | ... | 50·7 | 246·87 |
| | Mean | ...... | 246·26 | ... | ... | 50·1 | 246·88 |

* Clonmel, Darling Hill; garden adjoining the house.—Fermoy, field near the river.

† The first five observations, and those of December 19, 21, 23, 1835, made by Captain Sabine. All the others with this needle by Mr. Lloyd.

‡ Dublin, Provost's garden, Trinity College.—Armagh, grounds of the observatory.

## Table I.—(*Continued.*)

| | Date. | Hour. | Time. | | Rate | Ter | Corr.Time. |
|---|---|---|---|---|---|---|---|
| | | h m | | | | | |
| Carn ...... | Oct. 21. | 2 57 | 247·49 | 2·5 | +2·0 | 50·2 | 248·10 |
| Strabane ... | 23. | 1 22 | 248·00 | 2·5 | | 51·8 | 248·51 |
| Enniskillen . | 24. | 3 45 | 247·09 | 3·5 | | 38·5 | 248·42 |
| Dublin...... | 25. | 3 24 | 243·14 | 4·0 | | 45·7 | 244·01 |
| | 28. | 3 0 | 243·39 | 3·5 | | 53·0 | 243·82 |
| | | 3 26 | 243·49 | 4·5 | | 53·0 | 243·92 |
| | Mean | | 243·34 | | | 50·6 | 243·92 |
| Dublin...... | Aug. 19. | 36 | 243·81 | 3·0 | +7·3 | 72·0 | 243·06 |
| Markree .... | 21. | 2 35 | 249·30 | 3·5 | | 69·2 | 248·71 |
| Ballina....... | 22. | 3 | 249·90 | 2·0 | | 71·5 | 249·16 |
| Belmullet . | 24. | 1 50 | 250·40 | 1·5 | | 69·5 | 249·79 |
| Achill Ferry. | 25. | 12 0 | 249·35 | 2·0 | | 69·2 | 248·76 |
| Leenan .... | 26. | 3 58 | 248·19 | 2·0 | | 59·8 | 248·18 |
| Oughterard.. | 27. | 3 16 | 246·06 | 4·0 | | 57·5 | 246·19 |
| Ennis ....... | 28. | 3 42 | 244·44 | 2·0 | | 75·2 | 243·49 |
| Limerick..... | 29. | 52 | 243·45 | 3·5 | | 67·8 | 242·96 |
| Cork ....... | 31. | 1 46 | 241·50 | 2·5 | | 69·5 | 240·90 |
| Waterford . | Sept. 1. | 1 55 | 242·48 | 2·0 | | 69·0 | 241·92 |
| Broadway . | 2. | 3 15 | 241·24 | 2·0 | | 66·5 | 240·83 |
| | | 35 | 241·26 | 2·5 | | 66·2 | 240·87 |
| | Mean | | 241·25 | | | 66·4 | 240·85 |
| Rathdrum | 3. | 4 8 | 243·47 | 3·5 | | 63·0 | 243·27 |
| Dublin..... | 13. | 3 5 | 243·70 | 4·0 | +6·0 | 57·8 | 243·81 |
| | 14. | 2 29 | 243·92 | 4·0 | | 60·2 | 243·89 |
| | 15. | 2 36 | 243·14 | 2·5 | | 63·8 | 242·89 |
| | | 3 2 | 243·73 | 3·0 | | 63·5 | 243·50 |
| | Mean | | 243·62 | | | 61·3 | 243·52 |
| London .... | 19. | 11 48 | 236·51 | 3·0 | | 68·6 | 235·98 |
| | | 2 44 | 236·65 | 2·0 | | 69·4 | 236·07 |
| | | 12 43 | 236·63 | 3·5 | | 72·2 | 235·89 |
| | Mean | | 236·60 | | | 70·1 | 235·98 |

* Carn, an open field near the barracks.—Strabane, an open field adjoining the town.—Enniskillen, field near town.—Markree, demesne of the castle, part surrounded with tall trees.—Ballina, open field near town; no shelter from sun.—Belmullet, on the beach, at extremity of Broadhaven.—Achill Ferry, on the beach, near ferry.—Leenan, field at extremity of Killery harbour.—Oughterard, in a wood near the river.—Ennis, open field near town.—Limerick, garden at Somerville.—Cork, demesne on the banks of river, between Cork and Blackrock.—Waterford, demesne adjoining the river, side opposite town.—Broadway, open field.—Rathdrum, demesne of Avondale; deep wood.—London, Westbourne Green, Harrow Road.

TABLE I.—(*Continued.*)

| Place. | Date. | Hour. | Time. | Arc. | Rate. | Temp. | Corr. Time. |
|---|---|---|---|---|---|---|---|
| | | h m | " | ° | " | ° | " |
| London ...... | Oct. 23. | 1 18 | 235·09 | 3·0 | +7·8 | 52·4 | 235·52 |
| | 24. | 11 45 | 235·05 | 3·0 | ... | 54·5 | 235·35 |
| | | 2 23 | 235·12 | 3·5 | ... | 55·0 | 235·39 |
| | Mean | ...... | 235·09 | ... | ... | 54·0 | 235·42 |
| Dublin ......... | Nov. 5. | 1 46 | 243·18 | 3·0 | ... | 54·2 | 243·51 |
| | 6. | 12 21 | 243·35 | 3·0 | ... | 48·0 | 244·06 |
| | | 1 55 | 243·37 | 3·5 | ... | 48·7 | 244·04 |
| | Mean | ...... | 243·30 | ... | ... | 50·3 | 243·87 |
| Limerick ...... | Dec. 19. | 11 40 | 243·03 | ... | +4·0 | 45·5 | 243·90 |
| | 21. | 12 19 | 242·36 | ... | ... | 36·0 | 243·81 |
| | 23. | 11 17 | 242·68 | ... | ... | 33·0 | 244·32 |
| | Mean | ...... | 242·69 | ... | .. | 38·2 | 244·01 |
| Dublin ... ..... | Dec. 29. | 1 31 | 243·22 | 4·0 | +5·5 | 47·6 | 243·96 |
| | | 1 52 | 243·18 | 4·0 | ... | 47·0 | 243·95 |
| | Mean | ...... | 243·20 | ... | ... | 47·3 | 243·95 |
| Dublin ......... | Jan. 11. | 11 21 | 242·67 | 3·0 | ... | 33·8 | 244·24 |
| | | 11 44 | 242·48 | 4·0 | ... | 34·0 | 244·03 |
| | 12. | 10 12 | 242·86 | 2·0 | ... | 33·8 | 244·43 |
| | Mean | ...... | 242·67 | ... | ... | 33·9 | 244·23 |

*Time of Vibration of Cylinder* L (*b*)*.

| Place †. | Date. | Hour. | Time. | Arc. | Rate. | Temp. | Corr. Time. |
|---|---|---|---|---|---|---|---|
| | | h m | " | | " | ° | " |
| Limerick ...... | Sept. 9. | 2 14 | 292·55 | ... | 0·0 | 60·2 | 292·54 |
| Ballybunan ... | 17. | 10 3 | 292·25 | ... | ... | 62·0 | 292·10 |
| Glengariff ... | 27. | 12 24 | 293·15 | ... | ... | 68·0 | 292·57 |
| Killarney ...... | Oct. 4. | 12 38 | 292·15 | ... | ... | 68·0 | 291·57 |
| Limerick ...... | 8. | 1 38 | 292·51 | 4·0 | +15· | 62·0 | 292·31 |
| | | 2 5 | 292·92 | 4·0 | ... | 62·0 | 292·72 |
| | Mean | ...... | 292·72 | ... | ... | 62·0 | 292·52 |
| Dublin ........ | Oct. 11. | 1 34 | 292·82 | 1.5 | +1·0 | 62·5 | 292·63 |
| | | 2 50 | 292·73 | 2·5 | ... | 58·5 | 292·84 |
| | Mean | ...... | 292·78 | ... | ... | 60·5 | 292·74 |
| Carlingford ... | 13. | 1 45 | 294·65 | 3·0 | ... | 59·5 | 294·69 |
| Armagh ...... | 14. | 1 55 | 295·89 | 2·5 | ... | 51·0 | 296·56 |
| | 15. | 1 45 | 295·70 | 3·0 | ... | 52·5 | 296·25 |
| | Mean | ...... | 295·80 | ... | ... | 51·8 | 296·40 |

* First six observations, and those of 21st and 23rd of December 1835, made by Captain Sabine. All the others with this needle by Mr. Lloyd.

† Carlingford, open field east of town.

TABLE 1.—(*Continued.*)

| Place *. | Date. | Hour. | Time. | Arc. | Rate. | Temp. | Corr. Time. |
|---|---|---|---|---|---|---|---|
| | | h m | ″ | ° | ″ | ° | ″ |
| Colerain ...... | Oct. 18. | 4 3 | 293·64 | 3·0 | +2·0 | 47·5 | 294·55 |
| | 20. | 12 44 | 294·61 | 3·0 | ... | 57·6 | 294·78 |
| | Mean | ...... | 294·12 | ... | ... | 52·5 | 294·66 |
| Carn ......... | 21. | 2 28 | 297·07 | 2·0 | ... | 51·2 | 297·71 |
| Strabane ...... | 23. | 12 57 | 297·60 | 2·5 | ... | 51·5 | 298·22 |
| Enniskillen ... | 24. | 3 14 | 296·50 | 3·0 | ... | 42·0 | 297·83 |
| Dublin ......... | 25. | 4 0 | 292·70 | 3·0 | ... | 46·8 | 293·66 |
| | 28. | 2 36 | 293·14 | 3·5 | ... | 54·0 | 293·57 |
| | Mean | ...... | 292·92 | ... | ... | 50·4 | 293·62 |
| Dublin ......... | Aug. 19. | 3 3 | 293·70 | 2·0 | +7·3 | 72·0 | 292·80 |
| Markree...... | 21. | 12 45 | 300·14 | 2·0 | ... | 66·8 | 299·60 |
| | | 1 57 | 300·84 | 2·0 | ... | 72·0 | 299·91 |
| | Mean | ...... | 300·49 | ... | ... | 69·4 | 299·75 |
| Ballina ........ | 22. | 2 29 | 301·75 | ins. | .. | 75·7 | 300·53 |
| Belmullet ... | 24. | 12 26 | 302·23 | 1·0 | ... | 65·5 | 301·78 |
| | | 1 15 | 301·98 | 1·0 | ... | 69·0 | 301·27 |
| | Mean | ...... | 302·10 | ... | ... | 67·2 | 301·52 |
| Achill Ferry | 25. | 1 35 | 300·61 | 1·5 | ... | 68·0 | 299·98 |
| Leenan ...... | 26. | 3 32 | 298·65 | 1·5 | ... | 59·5 | 298·66 |
| Oughterard ... | 27. | 2 53 | 296·34 | 3·0 | ... | 58·2 | 296·44 |
| Ennis ......... | 28 | 3 18 | 294·61 | 1·5 | ... | 74·5 | 293·52 |
| Limerick ...... | 29. | 2 26 | 292·97 | 2·5 | ... | 67·8 | 292·38 |
| Cork ......... | 31. | 1 11 | 289·58 | 1·5 | ... | 68·2 | 288·97 |
| Waterford ... | Sept. 1. | 1 29 | 292·13 | 1·5 | ... | 68·2 | 291·51 |
| Broadway ... | 2. | 2 46 | 290·21 | 1·5 | ... | 67·5 | 289·64 |
| Rathdrum ... | 3. | 3 42 | 292·95 | 3·0 | ... | 63·5 | 292·67 |
| Dublin ......... | 12. | 4 41 | 293·16 | 3·0 | +6·0 | 58·2 | 293·27 |
| | 14. | 2 3 | 293·47 | 3·5 | ... | 61·0 | 293·38 |
| | 15. | 2 9 | 293·62 | 2·5 | ... | 64·5 | 293·27 |
| | Mean | ...... | 293·42 | ... | ... | 61·2 | 293·31 |
| London ...... | 19. | 11 20 | 284·46 | 2·0 | ... | 69·0 | 283·80 |
| | | 3 19 | 284·51 | 2·5 | ... | 66·2 | 284·05 |
| | 22. | 12 17 | 285·03 | 3·0 | ... | 71·8 | 284·17 |
| | Mean | ...... | 284·67 | ... | ... | 69·0 | 284·01 |
| London ...... | Oct. 23. | 12 53 | 282·65 | 2·5 | +7·8 | 52·2 | 283·17 |
| | 24. | 12 12 | 282·76 | 2·5 | ... | 52·8 | 283·24 |
| | | 12 33 | 282·84 | 3·0 | ... | 53·0 | 283·31 |
| | | 1 57 | 282·34 | 3·0 | ... | 54·2 | 282·72 |
| | Mean | ...... | 282·65 | ... | ... | 53·0 | 283·11 |
| Dublin ......... | Nov. 5. | 1 19 | 292·61 | 2·5 | ... | 55·0 | 292·95 |
| | 6. | 11 55 | 292·59 | 2·0 | ... | 48·8 | 293·38 |
| | | 2 23 | 292·67 | 3·0 | ... | 48·4 | 293·49 |
| | Mean | ...... | 292·62 | ... | ... | 50·7 | 293·27 |

* Colerain, demesne adjoining the town; spot near river surrounded by tall trees; local attraction apparently due to basalt.

Table I.—(*Continued.*)

| Place. | Date. | Hour. | Time. | Arc. | Rate. | Temp. | Corr. Time. |
|---|---|---|---|---|---|---|---|
| | | h m | ″ | ° | ″ | ° | ″ |
| Limerick...... | Dec. 21. | 1 6 | 291·10 | ... | +4.0 | 36·0 | 292·84 |
| | | 1 44 | 291·14 | ... | +4·0 | 36·0 | 292·88 |
| | 23. | 11 53 | 291·49 | ... | ... | 33·2 | 293·44 |
| | Mean | ...... | 291·24 | ... | ... | 35·1 | 293·05 |
| Dublin ......... | 29. | 12 44 | 292·42 | 2·5 | +5·5 | 49·7 | 293·15 |
| | | 1 8 | 292·19 | 2·5 | ... | 47·8 | 293·06 |
| | Mean | ...... | 292·30 | ... | ... | 48·8 | 293·10 |
| Dublin ........ | Jan. 11. | 10 55 | 291·85 | 2·0 | ... | 35·2 | 293·64 |
| | 12. | 10 37 | 291·81 | 2·0 | ... | 32·5 | 293·80 |
| | Mean | ...... | 291·83 | ... | ... | 33·8 | 293·72 |

*Time of Vibration of Cylinder* R (*c*)*.

| Place †. | Date. | Hour. | Time. | Arc. | Rate. | Temp. | Corr. Time. |
|---|---|---|---|---|---|---|---|
| | | h m | ″ | ° | ″ | ° | ″ |
| London ...... | July 8. | 7 34 | 440·08 | 1·5 | +1·4 | 58·0 | 440·22 |
| | | 10 9 | 440·57 | 1·5 | ... | 60·0 | 440·56 |
| | | 12 20 | 441·03 | 2·0 | ... | 64·0 | 440·72 |
| | | 4 17 | 441·33 | 2·0 | ... | 66·0 | 440·88 |
| | 9. | 11 26 | 441·93 | 1·0 | ... | 57·0 | 442·15 |
| | | 2 15 | 441·55 | 1·0 | ... | 61·0 | 441·46 |
| | | 6 10 | 441·06 | 1·0 | ... | 61·0 | 440·97 |
| | 10. | 11 7 | 441·76 | 1·5 | ... | 64·0 | 441·45 |
| | | 4 47 | 441·78 | 1·2 | ... | 62·0 | 441·62 |
| | 14. | 10 41 | 441·31 | 1·5 | ... | 58·0 | 441·45 |
| | 15. | 6 51 | 440·97 | 1·5 | ... | 54·0 | 441·42 |
| | | 11 11 | 443·26 | 1·0 | ... | 66·0 | 442·81 |
| | | 2 31 | 443·40 | 1·5 | ... | 70·0 | 442·64 |
| | | 8 24 | 440·86 | 1·5 | ... | 55·0 | 441 23 |
| | 16. | 7 39 | 441·19 | 1·0 | ... | 52·0 | 441·78 |
| | | 11 35 | 442·68 | 1·0 | ... | 64·0 | 442·37 |
| | | 8 2 | 441·53 | 1·0 | ... | 57·0 | 441·75 |
| | 18. | 10 14 | 443·32 | 1·0 | ... | 65·0 | 442·93 |
| | 19. | 7 35 | 441·00 | 1·5 | ... | 48·0 | 441·89 |
| | Mean | ...... | 441·61 | ... | ... | 60·1 | 441·59 |
| Limerick ...... | July 27. | 3 57 | 453·31 | 1·5 | +1·2 | 68·5 | 452·65 |
| | 28. | 12 39 | 454·60 | 2·0 | ... | 60·0 | 454·59 |
| | | 1 7 | 454·06 | 1·5 | ... | 59·0 | 454·13 |
| | | 1 40 | 454·18 | 2·0 | ... | 59·0 | 454·25 |

* Observed by Captain James Ross.

† London, Westbourne Green, Harrow Road.—Limerick, garden at Sonville.

TABLE I.—(*Continued.*)

| Place *. | Date. | Hour. | Time. | Arc. | Rate. | Temp. | Corr. Time. |
|---|---|---|---|---|---|---|---|
| | | h m | " | ° | " | ° | " |
| Limerick ...... | July 28. | 2 19 | 453·84 | 1·8 | +1·2 | 60·0 | 453·83 |
| | | 2 49 | 453·70 | 2·2 | ... | 59·0 | 453·77 |
| | | 3 22 | 453·82 | 2·0 | ... | 60·0 | 453·81 |
| | 29. | 11 0 | 454·00 | 2·2 | ... | 56·0 | 454·30 |
| | | 11 31 | 453·88 | 2·2 | ... | 57·0 | 454·10 |
| | | 12 0 | 454·04 | 2·0 | ... | 59·0 | 454·11 |
| | Mean | ...... | 453·94 | ... | ... | 59·8 | 453·95 |
| Dublin ......... | Aug. 16. | 7 21 | 453·90 | 2·0 | ... | 56·0 | 454·20 |
| | | 7 51 | 453·69 | 2·0 | ... | 56·0 | 453·99 |
| | | 8 21 | 453·77 | 2·0 | ... | 57·0 | 453·99 |
| | Mean | ...... | 453·79 | ... | ... | 56·3 | 454·06 |
| Markree ...... | Aug. 19. | 8 3 | 464·39 | 1·8 | ... | 54·0 | 464·85 |
| | | 10 0 | 465·17 | 2·0 | ... | 58·0 | 465·32 |
| | | 10 30 | 465·02 | 2·0 | ... | 58·0 | 465·17 |
| | Mean | ...... | 464·86 | ... | ... | 56·7 | 465·11 |
| London ...... | Aug. 30. | 9 41 | 440·72 | 2·5 | ... | 60·0 | 440·71 |
| | | 10 10 | 441·52 | 2·0 | ... | 61·0 | 441·43 |
| | | 10 38 | 441·72 | 2·0 | ... | 61·0 | 441·63 |
| | 31. | 9 33 | 440·86 | 2·0 | ... | 55·0 | 441·23 |
| | | 11 9 | 441·41 | 2·0 | ... | 58·0 | 441·55 |
| | | 12 37 | 442·04 | 2·0 | ... | 58·0 | 442·18 |
| | Mean | ...... | 441·38 | ... | ... | 58·5 | 441·46 |

*Time of Vibration of Cylinder* R (*d*) †.

| Place. | Date. | Hour. | Time. | Arc. | Rate. | Temp. | Corr. Time. |
|---|---|---|---|---|---|---|---|
| | | h m | " | ° | " | ° | " |
| London ...... | July 19. | 9 44 | 437·75 | 1·0 | +1·4 | 58·0 | 437·89 |
| | | 10 30 | 438·10 | 1·0 | ... | 59·0 | 438·17 |
| | | 11 44 | 438·39 | 1·2 | ... | 62·0 | 438·23 |
| | | 1 31 | 438·30 | 1·0 | ... | 64·0 | 438·00 |
| | | 3 6 | 439·18 | 1·0 | ... | 66·0 | 438·72 |
| | 20. | 7 19 | 438·55 | 1·0 | ... | 60·0 | 438·54 |
| | | 9 54 | 439·26 | 1·0 | ... | 61·0 | 439·18 |
| | | 10 39 | 440·06 | 1·0 | ... | 63·0 | 439·82 |
| | Mean | ...... | 438·70 | ... | ... | 61·6 | 438·57 |
| Limerick ...... | July 29. | 1 18 | 449·17 | 2·0 | +1·2 | 60·0 | 449·16 |
| | | 1 50 | 450·05 | 2·0 | ... | 61·0 | 449·96 |
| | | 2 41 | 449·16 | 2·0 | ... | 62·0 | 449·00 |

* Dublin, Provost's Garden, Trinity College.—Markree, demesne of castle; spot surrounded by lofty trees.

† Observed by Captain James Ross.

TABLE I.—(*Continued.*)

| Place. | Date. | Hour. | Time. | Arc. | Rate. | Temp. | Corr. Time. |
|---|---|---|---|---|---|---|---|
| | | h m | ″ | ° | ″ | ° | ″ |
| Limerick ...... | July 30. | 10 41 | 449·89 | 2·0 | +1·2 | 55·0 | 450·27 |
| | | 11 10 | 449·89 | 2·0 | ... | 56·0 | 450·19 |
| | | 11 40 | 449·80 | 1·5 | ... | 56·0 | 450·10 |
| | | 12 14 | 449·77 | 1·5 | ... | 57·0 | 449·99 |
| | 31. | 11 29 | 449·70 | 2·0 | ... | 54·0 | 450·15 |
| | | 11 58 | 449·76 | 2·0 | ... | 56·0 | 450·06 |
| | | 12 28 | 449·91 | 2·0 | ... | 57·0 | 450·13 |
| | | 12 58 | 449·92 | 2·0 | ... | 58·0 | 450·06 |
| | Mean | ...... | 449·73 | ... | ... | 57·5 | 449·92 |
| Dublin ......... | Aug. 14. | 6 36 | 451·82 | 2·5 | ... | 54·0 | 452·27 |
| | | 7 5 | 451·62 | 3·0 | ... | 54·0 | 452·07 |
| | | 7 35 | 451·61 | 3·0 | ... | 55·0 | 451·99 |
| | Mean | ...... | 451·68 | ... | ... | 54·3 | 452·11 |
| Markree ...... | Aug. 19. | 11 40 | 462·19 | 2·5 | ... | 59·0 | 462·26 |
| | | 12 10 | 462·00 | 2·5 | ... | 59·0 | 462·07 |
| | 20. | 8 37 | 462·54 | 2·5 | ... | 54·0 | 463·00 |
| | Mean | ...... | 462·24 | ... | ... | 57·3 | 462·44 |
| London ...... | Aug. 28. | 7 16 | 437·91 | 2·0 | ... | 53·0 | 438·43 |
| | | 7 56 | 438·42 | 2·5 | ... | 56·0 | 438·71 |
| | | 9 48 | 439·01 | 2·5 | ... | 60·0 | 439·00 |
| | 29. | 9 31 | 439·43 | 2·0 | ... | 60·0 | 439·42 |
| | | 10 13 | 439·52 | 2·0 | ... | 60·0 | 439·51 |
| | | 10 42 | 439·60 | 2·5 | ... | 63·0 | 439·36 |
| | Mean | ...... | 438·98 | ... | ... | 58·7 | 439·07 |

The computed results of the preceding observations are given in Table II. The first and second columns contain the place and the date of the observations; the third the name of the needle employed; in the fourth column is given the *mean time* of 100 vibrations, corrected for temperature and for the rate of the chronometer; and the fifth and sixth columns contain the computed values of the horizontal intensity;—the numbers in the fifth column being the ratios of the horizontal intensity at the place of observation to that at the station of the observer, and those in the sixth being the ratios of the same force to that at London, to which place all the observations are ultimately referred.

If T denote the reduced time of 100 vibrations at any place, and T' that at the station with which it is immediately compared, and if $h$ and $h'$ be the horizontal intensities at the two places, the numbers of the fifth column are computed from the formula

$$\frac{h}{h'} = \left(\frac{T'}{T}\right)^2.$$

Again, if $h_{\prime}$ denote the horizontal intensity in London, the ratio $\frac{h'}{h_{\prime}}$ will be determined in the same manner; and, multiplying by it the numbers in the fifth column, we obtain the values of $\frac{h}{h_{\prime}}$, or the ratios of the horizontal intensity at the places of observation to that at London, as given in the sixth and last column.

The stations with which all the other places in Ireland are immediately compared are Dublin and Limerick; and it will at once appear that, as the ratios of the horizontal force at these stations to that at London enter as factors in all the final results, much accuracy is required in their determination. For this purpose we have three distinct series of observations. In the first and second the intensities of the horizontal force in Dublin and Limerick are *directly* compared with that in London; and in the third these intensities are compared together. The results of these comparisons, given in Table II., are here put together, so as to be seen at one view.

I. Horizontal intensity in Dublin, the horizontal intensity in London being unity.

| | | | | |
|---|---|---|---|---|
| July, Aug. 1835 ........... | Cyl. | R (*c*). ...... | Int. = | ·9456 |
| ——————........... | — | R (*d*). ...... | — | ·9421 |
| September, 1835 ........... | — | L (*a*). ...... | — | ·9390 |
| ——————........... | — | L (*b*). ...... | — | ·9376 |
| Oct., Nov., 1835 ........... | — | L (*a*). ...... | — | ·9319 |
| ——————........... | — | L (*b*). ...... | — | ·9319 |
| | | | Mean = | ·9380 |

II. Horizontal intensity in Limerick, the horizontal intensity in London being unity.

| | | | | | | |
|---|---|---|---|---|---|---|
| July, Aug., Sept. 1834 ... | Cyl. | S (*b*). | ...... | Int. | = | ·9396 |
| July, 1835 .................. | — | — | ...... | — | | ·9470 |
| July, August 1835......... | — | R (*c*). | ...... | — | | ·9461 |
| ——————......... | — | R (*d*). | ...... | — | | ·9513 |
| | | | | Mean | = | ·9460 |

III. Horizontal intensity in Limerick, the horizontal intensity in Dublin being unity.

| | | | | | | | |
|---|---|---|---|---|---|---|---|
| Sept., Oct., 1834 | ......... | Cyl. | L (*a*). | ...... | Int. | = | 1·0064 |
| —————— | ......... | — | L (*b*). | ...... | — | | 1·0044 |
| July, Aug. 1835 | ......... | — | R (*c*). | ...... | — | | 1·0005 |
| —————— | ......... | — | R (*d*). | ...... | — | | 1·0098 |
| Aug., Sept. 1835 | ......... | — | L (*a*). | ...... | — | | 1·0027 |
| —————— | ......... | — | L (*b*). | ...... | — | | 1·0047 |
| Nov., Dec., 1835 | ......... | — | L (*a*). | ...... | — | | 1·0001 |
| —————— | ......... | — | L (*b*). | ...... | — | | 1·0021 |
| | | | | | Mean | = | 1·0038 |

If then $x$ and $y$ denote the horizontal intensities in Dublin and Limerick, that in London being unity, observation gives

$$x = \cdot 9380, \qquad y = \cdot 9460, \qquad \frac{y}{x} = 1\cdot 0038,$$

and it is required to determine the *most probable* values of $x$ and $y$. To generalize this problem, let the mean results of observation be $a$, $b$, $c$, and let their *weights* be A, B, C respectively; so that we have

$$\begin{aligned} x - a &= 0, \quad \text{weight} = \text{A}, \\ y - b &= 0, \quad \text{——} \quad \text{B}, \\ \frac{y}{x} - c &= 0, \quad \text{——} \quad \text{C}: \end{aligned}$$

$a$ and $b$ being *approximate* values of $x$ and $y$, let their true values be

$$x = a + \delta x, \quad y = b + \delta y,$$

and let $\frac{b}{a} = c_{\prime}$, then $\frac{y}{x} = \frac{b + \delta y}{a + \delta x} = c_{\prime} + a^{-1}(\delta y - c_{\prime}\delta x)$,

the squares and higher powers of the quantities $\delta x$ and $\delta y$ being neglected; so that the preceding equations may be written

$$\delta x = 0, \quad \delta y = 0,$$

$$a^{-1}(\delta y - c_{\prime}\delta x) + c_{\prime} - c = 0.$$

These three equations are to be combined by the method of least squares; that is, each equation is to be multiplied by its weight, and by the coefficient of each unknown quantity separately, and the two sets of results added together will be the final equations from which the values of the two unknown quantities are to be determined. We find in this manner

$$A\,\delta x - C\,c_{,}a^{-1}\,(a^{-1}\,\delta y - c_{,}a^{-1}\,\delta x + c_{,} - c) = 0$$

$$B\,\delta y + C\,a^{-1}\,(a^{-1}\,\delta y - c_{,}a^{-1}\,\delta x + c_{,} - c) = 0,$$

from which we obtain, by elimination,

$$\delta x = -\frac{\frac{B}{A}\,a\,c_{,}\,(c_{,} - c)}{\frac{B}{C}\,a^2 + \frac{B}{A}\,c^2 + 1} \qquad \delta y = -\frac{a\,(c_{,} - c)}{\frac{B}{C}\,a^2 + \frac{B}{A}\,c^2 + 1}.$$

If the weights of the three results be regarded as equal, that is, if

$$A = B = C,$$

the preceding values become

$$\delta x = \frac{b\,(c_{,} - c)}{a^2 + c_{,}^2 + 1}, \quad \delta y = -\frac{a\,(c_{,} - c)}{a^2 + c_{,}^2 + 1}.$$

To apply these results to the present case, we have

$$a = \cdot 9380, \quad b = \cdot 9460, \quad c = 1\cdot 0038$$

$$c_{,} = \frac{b}{a} = 1\cdot 0085, \quad c_{,} - c = \cdot 0047;$$

and introducing these values into the preceding expressions, we find

$$\delta x = -\,\delta y = \cdot 0015,$$

so that the corrected values of $x$ and $y$ are

$$x = \cdot 9395, \quad y = \cdot 9445.$$

## Table II.

*Intensity of the Horizontal Force.*

| Place. | Date. | Cyl. | Time. | Int. (1) | Int. (2) |
|---|---|---|---|---|---|
| Limerick ......... | July 23, 25, 1834. | S (*b*) | 400″·34 | | ·9396 |
| | Sept. 9. | ... | 403·63 | | |
| London ............ | Aug. 20—27. | ... | 389·66 | | 1·0000 |
| Limerick ......... | Sept. 9. Oct. 9, 29. | S (*b*) | 404·37 | 1·0000 | ·9445 |
| | Sept. 9. Oct. 8. | L (*a*) | 243·13 | 1·0000 | |
| | | L (*b*) | 292·53 | 1·0000 | |
| Ballybunan ...... | Sept. 16. | S (*b*) | 404·15 | 1·0010 | ·9443 |
| | 17. | L (*a*) | 243 69 | ·9954 | |
| | | L (*b*) | 292·10 | 1·0029 | |
| Glengariff ......... | 27. | S (*b*) | 402·14 | 1·0110 | ·9513 |
| | | L (*a*) | 241·80 | 1·0110 | |
| | | L (*b*) | 292·57 | ·9997 | |
| Killarney ......... | Oct. 4. | S (*b*) | 403·56 | 1·0039 | ·9505 |
| | | L (*a*) | 242·09 | 1·0086 | |
| | | L (*b*) | 291·57 | 1·0066 | |
| Tulla ............... | 12. | S (*b*) | 404·70 | ·9983 | ·9429 |
| Templemore ...... | 17. | ... | 396·43 | 1·0404 | ·9827 |
| Clonmel............ | 19. | ... | 402·50 | 1·0092 | ·9532 |
| Fermoy ............ | Dec. 2. | ... | 400·75 | 1·0157 | ·9593 |
| Limerick ......... | 10. | ... | 403·89 | 1·0000 | ·9445 |
| Dublin ............ | Oct. 10, 11, | L (*a*) | 243·90 | 1·0000 | ·9395 |
| | 25, 28. | ... | 243·92 | | |
| | 11. | L (*b*) | 292·74 | 1 0000 | ·9395 |
| | 25, 28. | ... | 293·62 | | |
| Limerick ......... | Sept. 9. Oct. 8. | L (*a*) | 243·13 | 1·0064 | |
| | | L (*b*) | 292·53 | 1·0044 | |
| Carlingford ...... | Oct. 13. | ... | 294·69 | ·9898 | ·9299 |
| Armagh ............ | 14, 15. | L (*a*) | 246·88 | ·9761 | ·9181 |
| | | L (*b*) | 296·40 | ·9784 | |
| Colerain ......... | 18, 20. | ... | 294·66 | ·9900 | ·9301 |
| Carn ............... | 21. | L (*a*) | 248·10 | ·9665 | ·9096 |
| | | L (*b*) | 297·71 | ·9698 | |
| Strabane ......... | 23. | L (*a*) | 248·51 | ·9633 | ·9066 |
| | | L (*b*) | 298·22 | ·9665 | |
| Enniskillen ...... | 24. | L (*a*) | 248·42 | ·9640 | ·9081 |
| | | L (*b*) | 297·83 | ·9690 | |
| London ............ | July 4—7, 1835 | S (*b*) | 401·33 | | 1·0000 |
| | July 8—19. | R (*c*) | 441·59 | | 1.0000 |
| | Aug. 30, 31. | ... | 441·46 | | |
| | July 19, 20. | R (*d*) | 438·57 | | 1·0000 |
| | Aug. 28, 29. | ... | 439·07 | | |
| Limerick ......... | July 27, 28. | S (*b*) | 412·38 | | ·9470 |
| | 27, 29. | R (*c*) | 453·95 | 1·0005 | ·9461 |
| | 29—31. | R (*d*) | 449·92 | 1·0098 | ·9513 |
| Dublin ............ | Aug. 16. | R (*c*) | 454·06 | 1·0000 | ·9456 |
| | 14. | R (*d*) | 452·11 | 1·0000 | ·9421 |

TABLE II.—(*Continued.*)

| Place. | Date. | Cyl. | Time. | Int. (1) | Int. (2) |
|---|---|---|---|---|---|
| Markree ......... | Aug. 19, 1835. | R (*c*) | 465″·11 | ·9531 | ·9012 |
| | 19, 20. | R (*d*) | 462·44 | ·9558 | ·9005 |
| Dublin ............ | Aug. 19. | L (*a*) | 243·06 | 1·0000 | ·9395 |
| | Sept. 12—15. | ... | 243·52 | | |
| | Aug. 19. | L (*b*) | 292·80 | 1·0000 | ·9395 |
| | Sept. 12—15. | ... | 293·31 | | |
| Markree ......... | Aug. 21. | L (*a*) | 248·71 | ·9569 | ·8986 |
| | | L (*b*) | 299·75 | ·9559 | |
| Ballina... ......... | 22. | L (*a*) | 249·16 | ·9534 | ·8946 |
| | | L (*b*) | 300·53 | ·9509 | |
| Belmullet ......... | 24. | L (*a*) | 249·79 | ·9486 | ·8894 |
| | | L (*b*) | 301·52 | ·9447 | |
| Achill Ferry ...... | 25. | L (*a*) | 248·76 | ·9565 | ·8977 |
| | | L (*b*) | 299·98 | ·9544 | |
| Leenan ........... | 26. | L (*a*) | 248.18 | ·9610 | ·9038 |
| | | L (*b*) | 298·66 | ·9629 | |
| Oughterard ...... | 27. | L (*a*) | 246·19 | ·9766 | ·9179 |
| | | L (*b*) | 296·44 | ·9773 | |
| Ennis.............. | 28. | L (*a*) | 243·49 | ·9984 | ·9373 |
| | | L (*b*) | 293·52 | ·9969 | |
| Limerick ......... | Aug. 29. | L (*a*) | 242·96 | 1·0027 | ·9430 |
| | | L (*b*) | 292·38 | 1·0047 | |
| Cork ............. | 31. | L (*a*) | 240·90 | 1·0199 | ·9582 |
| | | L (*b*) | 288·97* | 1·0285* | |
| Waterford......... | Sept. 1. | L (*a*) | 241·92 | 1·0114 | ·9498 |
| | | L (*b*) | 291·51 | 1·0107 | |
| Broadway ......... | 2. | L (*a*) | 240·85 | 1·0204 | ·9602 |
| | | L (*b*) | 289·64 | 1·0238 | |
| Rathdrum ......... | 3. | L (*a*) | 243·27 | 1·0002 | ·9409 |
| | | L (*b*) | 292·67 | 1·0027 | |
| Dublin ............ | 12—15. | L (*a*) | 243·52 | | ·9390 |
| | | L (*b*) | 293·31 | | ·9376 |
| London ............ | 19—22. | L (*a*) | 235·98 | | 1·0000 |
| | | L (*b*) | 284·01 | | 1·0000 |
| London ............ | Oct. 23, 24. | L (*a*) | 235·42 | | 1·0000 |
| | | L (*b*) | 283·11 | | 1·0000 |
| Dublin ............ | Nov. 5, 6. | L (*a*) | 243·87 | | ·9319 |
| | | L (*b*) | 293·27 | | ·9319 |
| Dublin ............ | Nov. Dec. Jan. | L (*a*) | 244·02 | 1·0000 | |
| | | L (*b*) | 293·36 | 1·0000 | |
| Limerick ......... | Dec. 19, 21, 23. | L (*a*) | 244·01 | 1·0001 | |
| | | L (*b*) | 293·05 | 1·0021 | |

* Disturbing influence suspected in this observation; the result has been therefore omitted in deducing the number in the last column.

## II. *Dip and Intensity.*

All the observations with dipping needles are comprised in the two Tables which follow. The first (Tab. III.) contains the results obtained with needles of the ordinary construction, and used exclusively for the determination of the dip. In the first, second, and third columns are given the *place*, *day of the month*, and *hour* of observation. The fourth column contains the observed inclination (the mean of the usual 8 readings) when the marked end of the needle is a *north pole*; the fifth contains the similar result of observation with the *poles reversed*; and the sixth is the mean of these angles, or the resulting *dip*. The needles employed are Needle L (1) constructed by Robinson, and Needle S (1) made by Dollond; the latter of these is $11\frac{1}{2}$ inches in length, the former $4\frac{1}{2}$ inches.

Table IV. contains the observations made for the purpose of determining the *dip* and *intensity* at the same time; the latter element being deduced from the direction in which the needle rests under the combined influence of magnetism and gravity, while the former is inferred from the position assumed under the influence of the earth's magnetism alone. Each of these angles of direction is deduced from the usual eight readings, all the reversals being made just as in the ordinary mode of observing the dip, the reversal of the poles of the needle excepted. These angles are given in the fifth and sixth columns of the table; $\zeta$ is the angle which the needle makes with the horizon when *unloaded*, and $\theta$ the inclination when a small weight is attached to the southern arm at a fixed distance from the centre. The temperature is noted at the commencement and end of each observation, with the view of correcting the value of the force; and the mean temperature is set down in the fourth column of the table. The needles employed in these observations are of the same dimensions as those used for the determination of the dip alone, and are adapted to the same divided circles. Three small holes are drilled close to each other on each arm, at a distance from the centre about two thirds of its length; and much care has been bestowed to make them coincide accurately with the axis of form of the needle. The weight is a small cylinder of brass, which is inserted in one of the holes on the southern arm, the diameter of the cylinder corresponding accurately to that of the hole. This weight is so adjusted as to bring the needle into a position nearly at right angles to the line of the dip, that being the position in which the resulting value of the force will be least affected by the friction of the axle on its supports.

## Table III.

*Observations of Dip.—Needle* L (1)*.

| Place. | Date. | Hour. | N. Pole. | S. Pole. | Dip. |
|---|---|---|---|---|---|
| | | h m | ° ′ | ° ′ | ° ′ |
| Limerick ... | July 1834. | | 70 59·4 | 71 1·8 | 71 0·6 |
| | | | 70 58·0 | 70 52·6 | 70 55·3 |
| | | | 71 3·6 | 70 59·8 | 71 1·7 |
| | | | 71 1·6 | 71 3·2 | 71 2·4 |
| | | | 70 56·0 | 70 58·4 | 70 57·2 |
| | Mean | ... | 70 59·7 | 70 59·2 | 70 59·5 |
| Dublin ...... | Aug. 7. | | 70 48·2 | 70 55·0 | 70 51·6 |
| | 8. | | 70 55·9 | 70 59·4 | 70 57·6 |
| | 9. | | 70 55·5 | 70 53·1 | 70 54·3 |
| | 19. | | 70 47·7 | 70 51·3 | 70 49·5 |
| | Sept. 22. | | 70 47·4 | 71 4·5 | 70 56·0 |
| | 23. | | 70 48·5 | 70 59·0 | 70 53·8 |
| | Mean | | 70 50·5 | 70 57·1 | 70 53·8 |
| Carlingford . | Oct. 13. | 12 20 | 71 2·0 | 71 30·6 | 71 16·3 |
| Armagh ...... | 14. | 12 30 | 71 27·2 | 71 33·1 | 71 30·2 |
| | 15. | 12 10 | 71 30·6 | 71 34·8 | 71 32·7 |
| | Mean | | 71 28·9 | 71 34·0 | 71 31·5 |
| Colerain † ... | 20. | 11 25 | 71 11·8 | 71 19·4 | 71 15·6 |
| Carn ......... | 21. | 12 45 | 71 48·2 | 71 47·5 | 71 47·8 |
| Strabane ... | 23. | 11 40 | 71 47·2 | 71 56·0 | 71 51·6 |
| Enniskillen . | 24. | 2 28 | 71 48·5 | 71 47·5 | 71 48·0 |
| Fermoy ...... | Dec. 2. | | 70 28·1 | 70 44·5 | 70 36·3 |
| Markree ... | Aug. 21. 1835. | 12 0 | 71 54·6 | 71 51·2 | 71 52 9 |
| | | 4 45 | 71 55·6 | 71 53·1 | 71 54·4 |
| | Mean | | 71 55·1 | 71 52·2 | 71 53·6 |
| Ballina ...... | Aug. 22. | 4 15 | 71 58·8 | 72 5·0 | 72 1·9 |
| Belmullet ... | 24. | 10 45 | 71 59·7 | 72 5·7 | 72 2·7 |
| Achill Ferry . | 25. | 10 0 | 71 52·2 | 71 56·6 | 71 54·4 |
| Galway ..... | 28. | 7 50 | 71 20·4 | 71 23·4 | 71 21 9 |
| Ennis ........ | | 4 20 | 70 55·6 | 71 7·4 | 71 1·5 |
| Limerick ... | 29. | 1 10 | 70 54·0 | 70 49·8 | 70 51·9 |
| Cork ......... | 31. | 12 0 | 70 26·5 | 70 32·1 | 70 29·3 |
| Waterford ... | Sept. 1. | 12 8 | 70 40·5 | 70 34·7 | 70 37·6 |
| Broadway ... | 2. | 1 30 | 70 14·8 | 70 24·0 | 70 19·4 |
| Gorey ...... | 3. | 8 5 | 70 45·0 | 70 41·8 | 70 43·4 |
| Rathdrum ... | | 2 30 | 70 39·9 | 70 42·3 | 70 41·1 |
| Dublin ...... | 4. | 1 30 | 70 44·4 | 70 49·0 | 70 46·7 |
| | 5. | 1 50 | 70 53·0 | 70 58·2 | 70 55·6 |
| | 7. | 3 35 | 70 56·0 | 70 52·5 | 70 54·2 |
| | 9. | 2 45 | 70 55·7 | 70 53·1 | 70 54·4 |
| | 14. | 12 35 | 70 55·9 | 70 57·5 | 70 56·7 |
| | 15. | 11 55 | 70 55·8 | 70 50·8 | 70 53·3 |
| | Mean | | 70 53·5 | 70 53·5 | 70 53·5 |

* The observations in Limerick (July 1834) and that in Fermoy (Dec. 2), were made by Captain Sabine: all the other observations with this needle by Mr. Lloyd.
† Evident local disturbance at this place: Rock, basalt.

## Table III.—(*Continued.*)

### *Needle* S (1)*.

| Place. | Date. | Hour. | N. Pole. | S. Pole. | Dip. |
|---|---|---|---|---|---|
| | | | ° ′ | ° ′ | ° ′ |
| Limerick ... | Aug. 1. 1834. | | 71 38·8 | 70 24·3 | 71 1·6 |
| | 16†. | | 70 48·5 | 71 22·3 | 71 5·4 |
| | Mean | | 71 13·6 | 70 53·3 | 71 3·5 |
| Glengariff ... | Sept. 27. | | 70 52·3 | 71 9·0 | 71 0·6 |
| | 28. | | 70 50·4 | 71 14·2 | 71 2·3 |
| | Mean | | 70 51·4 | 71 11·6 | 71 1·5 |
| Killarney ... | Oct. 4. | | 71 5·6 | 71 3·4 | 71 4·5 |
| Tulla ... ..... | 12. | | 71 16·2 | 71 15·4 | 71 15·8 |

* All the observations with Needle S (1) were made by Captain Sabine.

† The needle was rubbed on a hone in the interval between the observations (Aug. 1 and 16);—the marked end most.

## Table IV.

### *Observations of Dip and Intensity.—Needle* L (4) *.

| Place.† | Date. | | | Hour. | Temp. | (ζ.) | (θ.) |
|---|---|---|---|---|---|---|---|
| | | | | h m | ° | ° ′ | ° ′ |
| Limerick ... | June | 21. | 1834. | | 64·0 | | − 6 55·0 |
| | July | 22. | | | 65·0 | | − 7 10·6 |
| | | 28. | | | 65·0 | | − 6 21·3 |
| | | | Mean | | 64·7 | | − 6 49·0 |
| London ...... | Aug. | 28. | | 2 35 | 70·0 | 69 8·0 | −11 53·2 |
| | | 29. | | 12 42 | 68·5 | 69 8·5 | −12 7·4 |
| | | | | 1 18 | 67·7 | 69 5·6 | −12 26·1 |
| | | | Mean | | 68·7 | 69 7·4 | −12 8·9 |
| Dublin ...... | Sept. | 22. | | 2 15 | 61·0 | 71 2·2 | − 8 5·6 |
| | | 23. | | 2 45 | 62·5 | 70 53·8 | − 7 37·0 |
| | | 26. | | 2 40 | 66·2 | | − 7 59·6 |
| | | 29. | | 2 40 | 62·5 | 70 44·8 | − 8 9·5 |
| | | | Mean | | 63 0 | 70 53·6 | − 7 57·9 |
| Carlingford . | Oct. | 13. | | 12 52 | 61·2 | 71 20·6 | − 5 29·4 |
| Armagh ...... | | 14. | | 1 5 | 48·8 | 71 19·4 | − 7 7·4 |
| | | 15. | | 12 38 | 52·0 | 71 33·2 | − 7 15·2 |
| | | | Mean | | 50·4 | 71 26·3 | − 7 11·3 |
| Colerain ... | | 20. | | 12 2 | 56·3 | 71 12·2 | − 8 19·2 |
| Carn ......... | | 21. | | 1 28 | 49·5 | 71 49·6 | − 5 4·8 |
| Strabane ... | | 23. | | 12 18 | 48·8 | 71 39·4 | − 5 59·1 |
| Dublin ...... | | 25. | | 3 15 | 47·0 | 70 54·1 | − 8 53·9 |
| Dublin ...... | Aug. | 19. | 1835. | 1 13 | 71·5 | 70 51·6 | −13 4·9 |
| Markree ... | | 21. | | 3 50 | 67·0 | 71 55·6 | −11 2·6 |
| Ballina ...... | | 22. | | 3 50 | 66·5 | 71 51·8 | −11 15·2 |
| Belmullet ... | | 24. | | 11 15 | 65·5 | 71 57·5 | −10 56·2 |
| Achill Ferry . | | 25. | | 10 37 | 62·0 | 71 53·2 | −10 49·2 |
| Galway ...... | | 28. | | 8 18 | 59·0 | 71 17·4 | −11 9·1 |
| Ennis ......... | | | | 4 45 | 67·2 | 70 59·1 | −12 1·6 |
| Limerick ... | | 29. | | 1 40 | 69·2 | 70 47·5 | −12 32·0 |
| Cork ......... | | 31. | | 12 30 | 68·5 | 70 33·2 | −13 18·5 |
| Waterford ... | Sept. | 1. | | 12 35 | 66·2 | 70 38·8 | −13 38·6 |
| Broadway ... | | 2. | | 2 0 | 66·8 | 70 31·6 | −13 33·5 |
| Gorey ...... | | 3. | | 8 30 | 60·0 | 70 43·1 | −14 1·1 |
| Rathdrum ... | | | | 3 0 | 64·7 | 70 40·8 | −13 58·0 |

* The observations in Limerick (June, July 1834) were made by Captain Sabine; all the other observations with this needle by Mr. Lloyd.

† London, Sir James South's observatory, Kensington.

TABLE IV.—(*Continued.*)

| Place.* | Date. | | | Hour. | Temp. | (ζ.) | (θ.) |
|---|---|---|---|---|---|---|---|
| | | | | h m | ° | ° ′ | ° ′ |
| Dublin ...... | Sept. | 4. | 1835. | 1 56 | 71·8 | 70 43·6 | −12 29·4 |
| | | 5. | | 2 19 | 65·5 | 70 52·8 | −13 15·4 |
| | | 7. | | 4 8 | 70·0 | 70 52·2 | −13 20·8 |
| | | 9. | | 3 5 | ... | 70 46·2 | |
| | | 14. | | 1 5 | ... | 70 53·4 | |
| | | 15. | | 12 25 | 62·0 | 70 55·0 | −13 10·5 |
| | | | Mean | ... | 67·3 | 70 50·5 | −13 4·0 |
| London ... | Sept. | 19. | | 1 0 | ... | 69 5·8 | |
| | | | | 1 10 | 68·0 | 69 7·4 | −16 54·2 |
| | | 22. | | 11 32 | 70·8 | 69 12·4 | −17 16·3 |
| | | | | 1 45 | 70·0 | 69 13·6 | −16 49·4 |
| | | | Mean | ... | 69·6 | 69 9·8 | −17 0·0 |
| London ...... | Oct. | 23. | | 11 33 | 50·5 | 69 10·6 | −16 32·0 |
| | | | | 2 25 | 51·6 | 69 2·2 | −16 35·4 |
| | | 24. | | 1 16 | 53·8 | 69 6·0 | −16 44·4 |
| | | | Mean | ... | 52·0 | 69 6·3 | −16 37·3 |
| Dublin ...... | Nov. | 5. | | 12 22 | 56·2 | 70 49·6 | −12 54·6 |
| | | | | 2 32 | 52·8 | 70 45·8 | −12 54·5 |
| | | 6. | | 1 15 | 49·0 | 70 53·9 | −12 36·6 |
| | | | Mean | | 52·7 | 70 49·8 | −12 48·6 |

*Needle* S (2) †.

| Place.* | Date. | | | Hour. | Temp. | (ζ.) | (θ.) |
|---|---|---|---|---|---|---|---|
| Limerick ‡... | July. | | 1835. | | 63·0 | 71 16·9 | −15 9·0 |
| Ballybunan . | Nov. | 8. | | | 52·0 | 71 29·1 | −13 56·3 |
| Valentia...... | | 12. | | | 47·0 | 71 15·0 | −14 37·3 |
| Dingle ...... | | 18. | | | 43·0 | 71 17·7 | −13 45·8 |
| Tulla ......... | Dec. | 10. | | | 47·0 | 71 36·5 | −14 46·0 |
| Limerick § ... | | 26, 27 | | | 45·0 | 71 14·6 | −15 29·6 |
| Youghal ‖ ... | | 29. | | | 47·0 | 70 49·0 | −16 0·5 |
| Limerick ... | Jan. | 4. | 1836. | | 52·5 | | −15 23·7 |

* Limerick, garden at Somerville.—Ballybunan, in the field in front of Capt. Raymond's Lodge.—Valentia, on the sea beach at "the Foot."—Dingle, on the sea beach at Lord Ventry's.—Tulla, Kiltanon, Mr. Molony's demesne.—Youghal, in the garden of the "Devonshire Arms" inn.

† The observations in Limerick, July 1835, made by Captains Sabine and Ross; all the remaining observations with this needle by Captain Sabine.

‡ ζ a mean of 4 observations; θ mean of 3 obs.

§ ζ a mean of 3 obs.; θ mean of 2 obs.

‖ Mean of 2 obs.

When the observations of dip made at the same station with different needles are compared together, it will be found that they are by no means in accordance. Thus the dip at Limerick in November, 1833, deduced from four observations with a needle on Meyer's principle, was 71° 11'·7, while the mean of five observations with needle L (1) at the same place and in the following year was only 70° 59'·5, differing from the former by 12'. When from this difference the amount of the annual change is deducted, the remainder appears to be greater than can be fairly ascribed to the errors of observation. But these discrepancies in the results given by different needles have been placed in the strongest light by the recent observations of Captain James Ross in London. In these observations, which were undertaken with the view of determining the amount of the annual decrease of dip at London, eight different needles were employed, and from eight to ten observations were made with each, the result of each separate observation being a mean of eighty readings. The results were as follow:

| | | | | |
|---|---|---|---|---|
| Needle B (1), Admiralty . . | (10 obs.) . . . | dip = | 69° | 1'·5 |
| Needle L (1) . . . . . . . . . | (9 obs.) . . . | —— | 69 | 6·3 |
| Needle S (1) . . . . . . . . . | (8 obs.) . . . | —— | 69 | 11·3 |
| Needle J . . . . . . . . . . . | (10 obs.) . . . | —— | 69 | 16·1 |
| Needle R . . . . . . . . . . . | (10 obs.) . . . | —— | 69 | 18·9 |
| Meyer's needle . . . . . . . | (8 obs.) . . . | —— | 69 | 19·6 |
| Needle B (2), Admiralty . . | (10 obs.) . . . | —— | 69 | 21·8 |
| Needle P . . . . . . . . . . . | (8 obs.) . . . | —— | 69 | 42·6 |
| | | Mean dip = | 69 | 17·3 |

Thus it appears that there is a difference amounting to 41' in the results of two of the needles used; and that this difference is very far beyond the limits of the errors of observation will appear from the fact that the *extreme difference* in the *partial* results with one of these needles (B (1)) does not amount to four minutes and a half, while with the other (P) the extreme difference is only two minutes. In fact, it so happens that these very needles which differ most widely in their *mean* results, are those in which the accordance of the *partial* results is most complete. Of the eight results obtained with needle (P), there is one only which differs from the mean of the eight by a single minute; and yet the mean of all the observations with this needle differs by more than 20' from the mean of any of the others, while its excess above the mean of the entire series amounts to 25'.

These differences cannot be ascribed to any partial magnetism in the apparatus, for three of the needles (J, P, and R) were of

the same dimensions and were used with the same circle, and yet their results, as we see, are widely discordant. We must seek, then, in the needles themselves the cause of these perplexing discrepancies, and we are forced to conclude that there may exist, even in the best needles, some source of constant error which remains uncorrected by the various reversals usually made; and that accordingly no repetition of observations with a needle so circumstanced can furnish even an approximation to the absolute dip. If this error be due to the incomplete adjustment of the needle (such as deviation of centre of gravity from the axle, &c.) its magnitude will be a function of the dip, and of the force, which may be assumed to be *constant* where the variations of these elements are not considerable. Hence, to determine its amount for any particular needle, it is necessary to make a careful series of observations with it at some station for which the dip has been accurately determined (from the mean of several needles); and the difference will be a constant correction to be applied to all future results within certain limits.

It fortunately happens that the two ordinary needles used in the present series of observations in Ireland were among those employed by Captain James Ross in London; so that their corrections may be considered to be accurately known. The mean difference of the values of the dip as given by needles L (1) and L (4) having been well determined by observations elsewhere, the results obtained with the latter needle in London may be grouped with those of the former. Thus, the mean of seven observations made with needle L (4), September and October, 1835, (when reduced to needle L (1),) is 69° 9′·8. If then we combine this with the direct result of the nine observations with needle L (1), viz. 69° 6′·3, (allowing *double weight* to each of the latter observations on account of the double number of readings,) we find 69° 7′·3 as the mean value of the dip deduced from sixteen observations with the two needles, and reduced to needle L (1) as the standard. Comparing this with the mean result of the eight needles, the *correction* of needle L (1) is found to be + 10′·0.

For the other needles employed in Ireland, we have

Needle S (1) . . . . correction = + 6′·0.
Meyer's needle . . . . . . . . . . — 2′·3.

With respect to Meyer's needle, however, it is to be observed that as the angles from which the dip is deduced differ in general very widely, and as these angles are usually varied in different observations with the same needle, there is a presumption,

at least, that every constant error will be removed by repetition, and that the differences of the separate results from the absolute dip will be equal on the positive and on the negative side. This seems to be confirmed by the *amount* of the final difference in the present instance, which does not appear to be larger than may be fairly ascribed to the errors of observation. It seems better therefore to regard this needle as subject to no constant error.

The degree of confidence to which these determinations are entitled, may now be estimated by applying the corrections so obtained to the observations made with these needles at Limerick in 1833 and 1834, the only other station at which they have been all employed. The observations in the former year are reduced to the latter, assuming the annual decrease of dip in Ireland to be 3′. The very close agreement of the results must of course be regarded as in a great measure accidental.

| | Obs. Dip. | Corr. Dip. |
|---|---|---|
| Needle L (1), July 1834 | 70° 59′·5 | 71° 9′·5 |
| Needle S (1), Aug. 1834 | 71 3·5 | 71 9·5 |
| Meyer's needle, Nov. 1833 | 71 11·7 | 71 9·7 |

We have hitherto spoken only of the needles whose poles are changed in each observation, and which are used exclusively for the determination of the dip. The necessity of a correction in the results obtained with the other needles, whose poles are unaltered, is obvious. By reason of the deviation of the centre of gravity of the needle from the axle, the weight of the needle itself has in all cases a *certain moment* acting with or against the directive force.

Let $\zeta$, as before, be the inclination of the needle to the horizon when unloaded, and $\theta$ the corresponding angle when the weight is attached, and let $\rho$ denote the ratio of the moment of the needle itself to that of the added weight; then the dip ($\delta$) will be given by the equations*.

$$\delta = \zeta + \varepsilon$$

$$\sin \varepsilon = \rho \frac{\cos \zeta}{\cos \theta} \sin (\zeta - \theta). \qquad (1)$$

in which $\varepsilon$ is the correction sought.

The constant coefficient $\rho$ in the expression for this correction will be known when the corresponding values of the angles $\delta$, $\zeta$, and $\theta$ are known at some one station. Its value, in the case of Needle IV., has been thus found to be ·00205†.

* *Trans. Royal Irish Academy*, vol. xvii. p. 450. † Ibid. p. 451.

It will easily appear, from the second of the preceding formulæ, that when the coefficient is so small as that just assigned, the variations in the values of $\varepsilon$, resulting from moderate changes in the angles on which it depends, will be inconsiderable. In the observations in Ireland, for example, the entire change in the amount of the correction is a small fraction of a minute. In this and other similar cases, therefore, the correction may be regarded as *constant*; and its value may be inferred from any series of simultaneous observations made with the needle to be corrected, and with some other whose correction is already known. In this manner it has been found that the mean difference of the results of needles L (1) and L (4) is $\delta_{\mathrm{I}} - \delta_{\mathrm{IV}} = + 1'{\cdot}5$; being somewhat smaller than that assigned above. But if $\delta$ denote the *absolute* dip, we have already found that $\delta - \delta_{\mathrm{I}} = + 10'{\cdot}0$; and adding these differences, the correction of needle L (4) is $\delta - \delta_{\mathrm{IV}} = + 11'{\cdot}5$.

The correction of needle S (2) is inferred from the observations made with that needle in Limerick, as given in Table IV.

| | | | | |
|---|---|---|---|---|
| Limerick, July 1835 | . . . | dip = | 71° | 16′·9 |
| Dec. | . . . | | 71 | 14·6 |
| Mean | . . . | | 71 | 15·8 |

This mean corresponds, in time, to the middle of October 1835. But the true dip in Limerick (July, August, 1834,) was found to be $71° 9'{\cdot}5$; and when reduced to October 1835 (assuming the annual decrease to be $3'$,) it is $71° 6'{\cdot}0$. The correction of the needle is therefore $- 9'{\cdot}8$.

The corrections of the needles being determined, we may now proceed to deduce the values of the absolute dip at the several places at which observations have been made. These values are given in the following table (Table V.). In the first and second columns are written the place and date of the observation. The third and fourth columns contain the *corrected* values of the dip deduced from the results of Tables III. and IV. by the application of the corrections now explained, and the fifth and last column contains the mean dip inferred from the two preceding. In taking this mean, *double weight* has been assigned to the results obtained with needles of the ordinary construction, the number of readings with these needles being double of that made with the needles whose poles are unaltered.

## Table V.

### *Dip.—Final Results.*

| Place. | Date. | Dip (1). | Dip (2). | Mean. |
|---|---|---|---|---|
| | | ° ′ | ° ′ | ° ′ |
| London ......... | August 1834. | ......... | 69 18·9 | 69 18·9 |
| Dublin ......... | Aug., Sept. | 71 3·8 | 71 5·1 | 71 4·1 |
| Limerick ......... | July, Aug. | 71 9·5 | ......... | 71 9·5 |
| Glengariff ..... | Sept. 27, 28. | 71 7·5 | ......... | 71 7·5 |
| Killarney ......... | Oct. 4. | 71 10·5 | ......... | 71 10·5 |
| Tulla ............ | 12. | 71 21·8 | ......... | 71 21·8 |
| Carlingford ...... | 13. | 71 26·3 | 71 32·1 | 71 28·2 |
| Armagh ......... | 14, 15. | 71 41·5 | 71 37·8 | 71 40·3 |
| Colerain ......... | 20. | 71 25·6 | 71 23·7 | 71 25·0 |
| Carn ............ | 21. | 71 57·8 | 72 1·1 | 71 58·9 |
| Strabane ......... | 23. | 72 1·6 | 71 50·9 | 71 58·0 |
| Enniskillen ...... | 24. | 71 58·0 | ......... | 71 58·0 |
| Fermoy ......... | Dec. 2. | 70 46·3 | ......... | 70 46·3 |
| London ......... | Sept., Oct. 1835. | 69 16·3 | 69 19·8 | 69 17·3 |
| Dublin ......... | Sept. 4—15. | 71 3·5 | 71 2·0 | 71 3·0 |
| Markree ......... | Aug. 21. | 72 3·6 | 72 7·1 | 72 4·8 |
| Ballina ......... | 22. | 72 11·9 | 72 3·3 | 72 9·0 |
| Belmullet ...... | 24. | 72 12·7 | 72 9·0 | 72 11·5 |
| Achill Ferry ... | 25. | 72 4·4 | 72 4·7 | 72 4·5 |
| Galway ......... | 28. | 71 31·9 | 71 28·9 | 71 30·9 |
| Ennis ............ | | 71 11·5 | 71 10·6 | 71 11·2 |
| Limerick ......... | 29. | 71 1·9 | 70 59·0 | 71 0·9 |
| Cork ............ | 31. | 70 39·3 | 70 44·7 | 70 41·1 |
| Waterford ...... | Sept. 1. | 70 47·6 | 70 50·3 | 70 48·5 |
| Broadway ...... | 2. | 70 29·4 | 70 43·1 | 70 34·0 |
| Gorey ............ | 3. | 70 53·4 | 70 54·6 | 70 53·8 |
| Rathdrum ...... | | 70 51·1 | 70 52·3 | 70 51·5 |
| Dublin ......... | Nov. 5, 6. | ......... | 71 1·3 | 71 1·3 |
| Limerick ......... | July, Dec. | ......... | 71 6·0 | 71 6·0 |
| Ballybunan ...... | Nov. 8. | ......... | 71 19·3 | 71 19·3 |
| Valentia ......... | 12. | ......... | 71 5·2 | 71 5·2 |
| Dingle ............ | 18. | ......... | 71 7·9 | 71 7·9 |
| Tulla ............ | Dec. 10. | ......... | 71 26·7 | 71 26·7 |
| Youghal ......... | 29. | ......... | 70 39·2 | 70 39·2 |

The dip being known, the intensity will be given by the formula

$$\phi \sin (\delta - \theta) = \beta \cos \theta, \qquad (2)$$

in which $\beta$ is constant, and $\phi$ the measure of the force exerted by the earth on the needle. This force, however, varies with the temperature to which the needle is exposed; and it is necessary to determine the amount of this variation before we can know the relative values of the terrestrial magnetic force at different stations. Let $t$, then, be the *observed*, and $t'$ the *standard* temperature, and let $\phi'$ be the value of $\phi$ corresponding to the latter; then

$$\phi - \phi' = - \alpha \phi' (t - t'), \qquad (3)$$

in which $\alpha$ is a constant to be determined by observation. For needle L [*] it has been found that

$$\alpha = \cdot 00016.$$

But we may proceed in another way, which will perhaps be found convenient in practice. We may *correct* the observed value of $\theta$ by subtracting the change due to temperature; or, in other words, we may reduce the value of $\theta$ to that corresponding to the standard temperature, and to the standard condition of the needle. For this purpose it is only necessary to find the relation between the corresponding changes of $\phi$ and $\theta$. Differentiating, therefore, the equation (2) with respect to these variables, and dividing the result by the equation itself, we find

$$\frac{d\,\phi}{\phi\,d\,\theta} = \frac{\cos\delta \sin 1'}{\cos\theta \sin(\delta - \theta)},$$

$d\,\theta$ being expressed in minutes. Now it is easy to see that the variations of the second member of this equation (arising from changes in the angles $\delta$ and $\theta$ on which it depends) will be inconsiderable for the limited extent of those changes in Ireland. Assuming it to be constant, therefore, its value will be given when we know the corresponding values of $\delta$ and $\theta$ at some one station. Thus, at Dublin, September 1835, it was found that

$$\delta = 71^\circ\, 3'{\cdot}0, \quad \theta = -13^\circ\, 0'{\cdot}0;$$

from which we find the value of this constant to be ·00010. But, since $d\,\phi = \phi - \phi' = -\alpha\,\phi'\,(t - t')$, the first member of the equation is

$$-\alpha\,\frac{t - t'}{\theta - \theta'} = -\ \cdot 00016\,\frac{t - t'}{\theta - \theta'};$$

so that the correction is finally

$$\theta' - \theta = +\,1{\cdot}6\,(t - t')\,\dagger.$$

Now if $\phi_{\prime}$, $\delta_{\prime}$, and $\theta_{\prime}$ be the values of $\phi$, $\delta$, and $\theta$ at the station with which the rest are compared, we have

$$\phi \sin(\delta - \theta) = \beta \cos\theta,$$
$$\phi_{\prime} \sin(\delta_{\prime} - \theta_{\prime}) = \beta \cos\theta_{\prime};$$

and dividing
$$\frac{\phi}{\phi_{\prime}} = \frac{\cos\theta \sin(\delta_{\prime} - \theta_{\prime})}{\cos\theta_{\prime} \sin(\delta - \theta)}; \qquad (4).$$

* *Trans. Royal Irish Academy*, vol. xvii. p. 452.

† It is obvious that the coefficient in this correction might have been determined *directly*, by observing the angles $\theta$ and $\theta'$ corresponding to very unequal temperatures. It did not seem safe, however, to subject the apparatus to the action of high artificial heat, and the thermo-electric currents induced by inequality of temperature would in all probability have sensibly affected the results.

which expresses the ratio of the force at the two stations in a form suited to logarithmic calculation.

The following table gives the results of this computation. It contains the *place* and *date* of observation; the *angle* $\theta$ corrected for temperature; and the *total intensity* at each station, compared in the first instance with Dublin or Limerick, and in the second with London.

For the intensity of the magnetic force at Dublin we have the three following determinations:

| | | |
|---|---|---|
| Aug., Sept. 1834 | . . Int. = | 1·0194 |
| Sept. 1835 | . . . . . . . . . . . | 1·0213 |
| Oct., Nov. 1835 | . . . . . . . | 1·0211 |
| Mean | . . . . . = | 1·0206 |

The intensity at Limerick, compared with London, is observed to be 1·0262; and the intensity at the same place, compared with Dublin, is 1·0030. Accordingly, for the determination of the values of the total force at Dublin and Limerick, observation furnishes us with three results, in the two former of which the intensities at these two stations are directly compared with that at London, while in the third they are compared together. To infer from these data, therefore, the most probable values of the force at the two stations, we must proceed precisely as in the analogous problem respecting the horizontal intensities, and we have only to substitute in the formulæ already given*, for $a$, $b$, $c$, &c. their particular values. We have then

$$a = 1{\cdot}0206, \quad b = 1{\cdot}0262, \quad c = 1{\cdot}0030,$$

$$\frac{b}{a} = 1{\cdot}0055, \quad c_{,} - c = {\cdot}0025.$$

And since the comparison of Dublin and London is the mean of three distinct comparisons, while each of the other two results is inferred from one only, the *weights* may be assumed as follow:

$$\mathrm{A} = 3, \quad \mathrm{B} = 1, \quad \mathrm{C} = 1.$$

Substituting these values, therefore, in the formulæ alluded to, we find

$$\delta x = +{\cdot}0004, \quad \delta y = -{\cdot}0011;$$

$$x = a + \delta x = 1{\cdot}0210, \quad y = b + \delta y = 1{\cdot}0251.$$

The numbers in the fifth column of the table are deduced from those in the fourth, by multiplying by one or other of these numbers,—according as the force at the station in question has been compared in the first instance with that at Dublin or with that at Limerick.

* Page 134.

## TABLE VI.

### *Intensity.—Needle* L (4).

| Place. | Date. | Angle. | Int. (1.) | Int. (2.) |
|---|---|---|---|---|
| | | ° ′ | | |
| London......... | August 1834. | — 11 55·0 | ......... | 1·0000 |
| Dublin ......... | September. | — 7 53·1 | ......... | 1·0194 |
| Limerick ..... | June, July. | — 6 41·5 | ......... | 1·0262 |
| Dublin ......... | Sept., Oct. | — 8 33·9 | 1·0000 | 1·0210 |
| Carlingford*... | Oct. 13. | — 5 27·5 | 1·0166 | 1·0379 |
| Armagh ...... | 14, 15. | — 7 26·7 | 1·0044 | 1·0255 |
| Colerain* ...... | 20. | — 8 25·1 | ·9997 | 1·0207 |
| Carn ............ | 21. | — 5 21·6 | 1·0151 | 1·0364 |
| Strabane ...... | 23. | — 6 17·0 | 1·0100 | 1·0312 |
| Dublin ......... | Aug., Sept. 1835 | — 12 49·4 | 1·0000 | 1·0210 |
| Markree ..... | Aug. 21. | — 10 51·4 | 1·0091 | 1·0303 |
| Ballina ......... | 22. | — 11 4·8 | 1·0077 | 1·0289 |
| Belmullet ...... | 24. | — 10 47·4 | 1·0093 | 1·0305 |
| Achill Ferry . | 25. | — 10 46·0 | 1·0096 | 1·0308 |
| Galway ......... | 28. | — 11 10·7 | 1·0086 | 1·0298 |
| Ennis ......... | | — 11 50·1 | 1·0055 | 1·0266 |
| Limerick ...... | 29. | — 12 17·3 | 1·0030 | 1·0241 |
| Cork ........... | 31. | — 13 4·9 | ·9992 | 1·0202 |
| Waterford ... | Sept. 1. | — 13 28·7 | ·9966 | 1·0175 |
| Broadway...... | 2. | — 13 22·6 | ·9976 | 1·0185 |
| Gorey ......... | 3. | — 14 1·1 | ·9933 | 1·0142 |
| Rathdrum ... | | — 13 50·5 | ·9944 | 1·0153 |
| London......... | Sept. 19—22. | — 16 44·6 | ......... | 1·0000 |
| Dublin ......... | Sept. 4—15. | — 12 52·3 | ......... | 1·0213 |
| London ...... | Oct. 23, 24. | — 16 50·1 | ......... | 1·0000 |
| Dublin ......... | Nov. 5, 6. | — 13 0·3 | ......... | 1·0211 |

### *Needle* S (2).

| Place. | Date. | Angle. | Int. (1). | Int. (2.) |
|---|---|---|---|---|
| | | ° ′ | | |
| Limerick ...... | July, Dec. 1835. | — 15 19·3 | 1·0000 | 1·0251 |
| Ballybunan ... | Nov. 8. | — 13 56·3 | 1·0084 | 1·0337 |
| Valentia ...... | — 12. | — 14 37·3 | 1·0047 | 1·0299 |
| Dingle ......... | — 18. | — 13 45·8 | 1·0097 | 1·0350 |
| Tulla ......... | Dec. 10. | — 14 46·0 | 1·0035 | 1·0287 |
| Limerick ...... | Dec., Jan. | — 15 26·6 | 1·0000 | 1·0251 |
| Youghal ...... | Dec. 29. | — 16 0·5 | ·9971 | 1·0221 |

* Evident local disturbance at these two stations. The district about Carlingford is intersected with trap dykes; Colerain lies within the basaltic field of the North-east of Ireland.

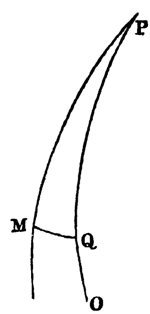
## III. *Isodynamic and Isoclinal Lines.*

On a review of the preceding results of observation, it will be seen that they exhibit much irregularity. The errors of observation (in which we are to include the effects of the unsteadiness of the magnetic state of the needles employed, as well as the various other uncertainties arising from the imperfections of our methods of observing,) have, of course, their share in these discrepancies; but they are by no means sufficient to explain the whole. The action of the earth on the magnetic needle is itself subject to irregularities, temporary as well as local; and it is to these that the observed anomalies must, in great part, be ascribed. Of the variations of the former kind we have already spoken. The direction and intensity of the terrestrial magnetic force, at a given place, are subject to fluctuations, or irregular oscillations round their mean state, the cause of which is as yet little understood; and it is only by means of simultaneous observations, made at some fixed station within the limits of the district through which these effects take place, that we can hope to ascertain their amount, and to correct for them. Of the local disturbing causes some are sufficiently obvious. Thus the needle is in general affected by the vicinity of basaltic rocks, owing to the quantity of iron they contain; and instances have been observed in which these rocks were even found to possess magnetic polarity*. But there seem to be grounds for believing that disturbing actions of a local nature are exerted on a much larger scale. Whether the earth's magnetic force be an inherent property, and the resultant of the forces of all its parts, or whether it be simply the effect of thermo-electric currents produced by the heating action of the sun, the result must in either case be greatly modified by the configuration of a country, and by the nature of its superficial strata. If this view be just, the greatest irregularities should prevail in those parts of the earth in which the uniformity of surface is broken by hill and valley, and where the strata have been rent and contorted by the uplifting of mountain chains. In Ireland, accordingly, we should expect to find much greater anomalies in the direction and intensity of the magnetic force than in the plains of central Europe; and it must be, consequently, in the same degree more difficult to arrive at general results.

* A remarkable case of this kind has been noticed at Fair Head, on the north coast of Ireland. The magnetic polarity of one of the columns which compose this wonderful façade is said to be so strong as to invert the position of the compass needle, when the poles of the same name are made to approach.

The only mode of escaping from these difficulties was to seek the general result of the entire series of observations, as to the position of the isodynamic and isoclinal lines; and to combine the partial results in such a manner that their deviations,—whether local, temporary, or casual,—should have the least influence on the final conclusion. Such is the object of the following computations.

Let $\lambda$ and $\mu$ denote the latitude and longitude of any place at which an observation has been made, $\lambda_{\prime}$ and $\mu_{\prime}$ the latitude and longitude of the station which is chosen as the origin of the coordinates; then the position of the former place may be fixed with reference to the latter in terms of these quantities. For let P be the pole of the earth, M and O the two places, P M and P O their meridians, and M Q a great circle passing through M and perpendicular to P O. It is obvious that the position of M will be determined by the rectangular spherical coordinates O Q and Q M. Now in the right-angled triangle M P Q, we have




$$\tan PQ = \tan PM.\cos P, \quad \sin MQ = \sin PM.\sin P;$$

or, denoting the coordinates O Q and Q M by $\alpha$ and $\beta$,

$$\left.\begin{aligned}\cot(\lambda_{\prime} + \alpha) &= \cot\lambda\cos(\mu - \mu_{\prime})\\ \sin\beta &= \cos\lambda\sin(\mu - \mu_{\prime}).\end{aligned}\right\}\quad (A)$$

When $\mu - \mu_{\prime}$ is so small as it is within the limits of the present district of observation, we may take

$$\sin(\mu - \mu_{\prime}) = \mu - \mu_{\prime}, \ \cos(\mu - \mu_{\prime}) = 1, \ \sin y = y,$$

and the preceding equations become

$$\left.\begin{aligned}\alpha &= \lambda - \lambda_{\prime},\\ \beta &= (\mu - \mu_{\prime})\cos\lambda.\end{aligned}\right\}\quad (B)$$

This simplification is obviously equivalent to the substitution of the parallel of latitude for the perpendicular to the meridian.

Now let us conceive any line to pass through O, making the angle $u$ with the meridian; then, in the same order of approximation, the perpendicular from the point M upon that line will be

$$p = \beta\cos u - \alpha\sin u;$$

and, substituting for $\alpha$ and $\beta$ their values just obtained,

$$p = (\mu - \mu_{\prime})\cos\lambda\cos u - (\lambda - \lambda_{\prime})\sin u. \qquad (C)$$

It is easy to see in what manner this result may be applied in obtaining equations of condition from the data furnished by observation. The increase of the force, or of the dip, may

(throughout the limited area of the present district of observation) be assumed to be proportional to the distance, measured in a direction perpendicular to the line of equal force, or of equal dip. Accordingly, if $u$ be the angle which the line of equal horizontal intensity passing through O makes with the meridian of the place, the difference of the intensities at the two stations will be proportional to $p$, or

$$h - h_{,} = r\,p\,;$$

$h$ and $h_{,}$ being the horizontal intensities at the two stations, and $r$ a constant coefficient which determines the rate of increase. Substituting, then, for $p$ its value (C), and making

$$r\cos u = x, \qquad r\sin u = y, \qquad \text{(D)}$$

we have

$$(\mu - \mu_{,})\cos\lambda\,.\,x - (\lambda - \lambda_{,})\,y = h - h_{,} \qquad \text{(E)}$$

The equations of condition deduced from the observations of total intensity, and of dip, will be of a similar form; and the coefficients of the unknown quantities, in the first member of the equations, will be the same.

The station chosen for the origin of the coordinates is Dublin, and it is obvious that there will be as many equations of condition as there are other places of observation. The coefficients of these equations are given in the following table. The first, second, and third columns contain the *place* of observation, its *latitude* and its *longitude**. The numbers in the fourth and fifth columns are the *differences* of latitude and longitude, (estimated in minutes of latitude,) of the place of observation and Dublin, or the values of $(\lambda - \lambda_{,})$ and $(\mu - \mu_{,})\cos\lambda$; and the numbers in the three remaining columns are the corresponding differences of *dip*, of *horizontal intensity*, and of *total intensity*, which form the second members of the equations. The dip having been observed at Dublin in each of the two years (1834 and 1835), the differences of dip are obtained by subtracting that belonging to the year in which the observation was made at the other station.

* The latitudes and longitudes of some of the more important stations have been kindly furnished by the officers of the Ordnance survey. The remainder have been taken from Arrowsmith's map of Ireland.

## Table VII.

| Place. | ($\lambda$) | ($\mu$) | ($\lambda - \lambda_{\prime}$) | ($\mu - \mu_{\prime}$) cos $\lambda$ | ($\delta - \delta_{\prime}$) | ($h - h_{\prime}$) | ($f - f_{\prime}$) |
|---|---|---|---|---|---|---|---|
| Carn .......... | 55° 15′ | 7° 15′ | +114 | + 34 | + 54′·9 | — ·030 | +·0154 |
| Colerain ...... | 55 8 | 6 40 | +107 | + 14 | + 21·0 | — ·009 | —·0003 |
| Strabane ...... | 54 49 | 7 28 | + 88 | + 42 | + 54·0 | — ·033 | +·0102 |
| Enniskillen ... | 54 21 | 7 38 | + 60 | + 48 | + 54·0 | — ·031 | |
| Armagh ......... | 54 21 | 6 39 | + 60 | + 14 | + 36·3 | — ·021 | +·0045 |
| Belmullet ...... | 54 20 | 9 50 | + 59 | +125 | + 68·5 | — ·050 | +·0095 |
| Markree ...... | 54 14 | 8 28 | + 53 | + 77 | + 61·8 | — ·039 | +·0093 |
| Ballina ......... | 54 10 | 9 3 | + 49 | + 98 | + 66·0 | — ·045 | +·0079 |
| Carlingford ... | 54 2 | 6 11 | + 41 | — 3 | + 24·2 | — ·010 | +·0169 |
| Achill Ferry ... | 54 0 | 9 51 | + 39 | +127 | + 61·5 | — ·042 | +·0098 |
| Leenan ......... | 53 41 | 9 40 | + 20 | +121 | | — ·036 | |
| Oughterard ... | 53 27 | 9 18 | + 6 | +109 | | — ·022 | |
| Dublin ......... | 53 21 | 6 15 | + 0 | + 0 | + 0·0 | + ·000 | +·0000 |
| Galway ......... | 53 17 | 8 51 | — 4 | + 93 | + 27·9 | | +·0088 |
| Rathdrum ...... | 52 55 | 6 12 | — 26 | — 2 | — 11·5 | + ·001 | —·0057 |
| Tulla * ......... | 52 53 | 8 41 | — 28 | + 88 | + 17·8 | + ·003 | +·0077 |
| Ennis ............ | 52 52 | 8 54 | — 29 | + 96 | + 8·2 | — ·002 | +·0056 |
| Limerick * ...... | 52 40 | 8 36 | — 41 | + 85 | + 5·0 | + ·005 | +.0041 |
| Gorey ......... | 52 41 | 6 15 | — 40 | + 0 | — 9·2 | | —.0068 |
| Clonmel ...... | 52 20 | 7 41 | — 61 | + 53 | | + ·013 | |
| Ballybunan ... | 52 35 | 9 34 | — 46 | +121 | + 16·3 | + ·004 | +·0127 |
| Broadway ...... | 52 14 | 6 20 | — 67 | + 3 | — 29·0 | + ·021 | —·0025 |
| Waterford...... | 52 12 | 7 6 | — 69 | + 31 | — 14·5 | + ·010 | —·0035 |
| Dingle ......... | 52 6 | 10 20 | — 75 | +150 | + 4·9 | | +·0140 |
| Killarney ...... | 52 3 | 9 31 | — 78 | +121 | + 6·5 | + ·011 | |
| Fermoy ......... | 52 1 | 8 34 | — 80 | + 86 | — 17·7 | + ·019 | |
| Valentia......... | 51 56 | 10 12 | — 85 | +146 | + 2·2 | | +·0089 |
| Cork ............ | 51 54 | 8 28 | — 87 | + 83 | — 21·9 | + ·019 | —·0008 |
| Youghal......... | 51 53 | 7 51 | — 88 | + 59 | — 23·8 | | +.0011 |
| Glengariff ...... | 51 44 | 9 33 | — 97 | +123 | + 3·5 | + ·011 | |

The equations of condition (E) are of the first dimension with respect to the two unknown quantities they contain, and may be written

$$a\,x + b\,y = c; \qquad \text{(F)}$$

in which the values of $a$, $b$, and $c$ (or of $(\mu - \mu_{\prime})\cos\lambda$, $\lambda - \lambda_{\prime}$, and $h - h_{\prime}$,) are given in the preceding table. In order to deduce the most probable values of the two unknown quantities, these equations must be combined by the method of least squares. Accordingly, multiplying equation (F) by the coefficient of $x$ ($a$),

* Observations made in the year 1835 give Tulla $\delta - \delta_{\prime} = + 23'{\cdot}7$; Limerick, $\delta - \delta_{\prime} = - 2'{\cdot}1$.

and by the *weight* ($w$) of the determination which it represents, and adding the results, we have

$$S(w\,a^2)\,x + S(w\,a\,b)\,y = S(w\,a\,c); \qquad \text{(G)}$$

and, performing the same operation with respect to the coefficient of the other unknown quantity,

$$S(w\,a\,b)\,x + S(w\,b^2)\,y = S(w\,b\,c). \qquad \text{(G)}$$

These are the two final equations which, by elimination, will furnish the most probable values of the quantities sought.

Let the values of $x$ and $y$, obtained from these equations, be A and B; then substituting in (D),

$$r \cos u = \text{A}, \quad r \sin u = \text{B};$$

and, dividing, we have

$$\tan u = \frac{\text{B}}{\text{A}}; \qquad \text{(H)}$$

by which the direction of the isodynamic line is determined. Again, squaring and adding,

$$r = \sqrt{\text{A}^2 + \text{B}^2}; \qquad \text{(I)}$$

which gives the rate of increase of the force in the normal direction. The lines of absolute intensity, and of dip, will be obtained by a similar process, the only difference being in the values of the second members of the equations (F).

Before we can apply these formulæ to the investigation of the lines of horizontal intensity, it is necessary to assign the weights due to each equation of condition, or to the determination which it involves. We shall assume, accordingly, that the weights of the values of $(h - h_{\prime})$, recorded in the preceding table, are measured by the *number* of separate *comparisons* from which they have been deduced, and we shall have, on this principle,

| | | | | |
|---|---|---|---|---|
| Limerick | . . . | weight | = | 12, |
| Markree. | . . . | —— | = | 3, |
| Armagh . | . . . | —— | = | 2, |

the weights of each of the other determinations being represented by unity.

The values of $a$, $b$, and $c$ being given in Tab. VII., we may now proceed to calculate the coefficients of the equations (G). The elements of this calculation are given in the following table.

| Place. | $(w a^2)$ | $(w a b)$ | $(w b^2)$ | $(w a c)$ | $(w b c)$ |
|---|---|---|---|---|---|
| Carn ............ | 1156 | − 3876 | 12966 | −1·020 | +3·420 |
| Strabane ......... | 1764 | − 3696 | 7744 | −1·386 | +2·904 |
| Enniskillen ...... | 2304 | − 2880 | 3600 | −1·488 | +1·860 |
| Armagh ......... | 392 | − 1680 | 7200 | − ·588 | +2·520 |
| Belmullet ...... | 15625 | − 7375 | 3481 | −6·250 | +2·950 |
| Markree ......... | 17787 | −12243 | 8427 | −9·009 | +6·201 |
| Ballina............ | 9604 | − 4802 | 2401 | −4·410 | +2·205 |
| Carlingford ...... | 9 | + 123 | 1681 | + ·030 | + ·410 |
| Achill Ferry ... | 16129 | − 4953 | 1521 | −5·334 | +1·638 |
| Leenan ......... | 14641 | − 2420 | 400 | −4·356 | + ·720 |
| Oughterard ...... | 11881 | − 654 | 36 | −2·398 | + ·132 |
| Rathdrum ...... | 4 | − 52 | 676 | − ·002 | + ·026 |
| Tulla ... ........ | 7744 | + 2464 | 784 | + ·264 | + ·084 |
| Ennis ............ | 9216 | + 2784 | 841 | − ·192 | − ·058 |
| Limerick ......... | 86700 | +41820 | 20172 | +5·100 | +2·460 |
| Clonmel ......... | 2809 | + 3233 | 3721 | + ·689 | + ·793 |
| Ballybunan ...... | 14641 | + 5566 | 2116 | + ·484 | + ·184 |
| Broadway ...... | 9 | + 201 | 4489 | + ·063 | +1·407 |
| Waterford ...... | 961 | + 2139 | 4761 | + ·310 | + ·690 |
| Killarney......... | 14641 | + 9438 | 6084 | +1·331 | + ·858 |
| Fermoy ......... | 7396 | + 6880 | 6400 | +1·684 | +1·520 |
| Cork ............ | 6889 | + 7221 | 7569 | +1·577 | +1·653 |
| Glengariff ...... | 15129 | +11931 | 9409 | +1·353 | +1·067 |

Adding, we find,

$$S(w a^2) = 257431, \quad S(w a b) = +49169, \quad S(w b^2) = 116509,$$
$$S(w a c) = -23{\cdot}598, \quad S(w b c) = +35{\cdot}644.$$

And the equations are

$$257431\, x + 49169\, y = -23{\cdot}598$$
$$49169\, x + 116509\, y = +35{\cdot}644,$$

from which we have, by elimination,

$$x = -{\cdot}0001633 = \mathrm{A}$$
$$y = +{\cdot}0003748 = \mathrm{B}.$$

Finally, substituting these values in equations (H), (I),

$$\tan u = -2{\cdot}2952, \quad u = -66^\circ\, 28', \quad r = -{\cdot}000409.$$

The positive branches of the axes of coordinates having been assumed to be those which stretch to the north and to the west, it follows that the lines of equal horizontal intensity lie to the east of north, making an angle of $66\frac{1}{2}^\circ$ nearly with the meridian of Dublin; the horizontal intensity decreases as we proceed northward, the decrease being equal to the distance traversed in a direction perpendicular to these lines (estimated in geographical miles or minutes of latitude) multiplied by the coefficient ·000409. The lines are laid down in the accompany-

ing chart for differences of ·01 in the value of the intensity, the corresponding intervals of distance being 24·4 geographical miles.

On a comparison of the separate determinations with the resulting lines, it will be observed that the intensities in the northern group are greater than those due to their position, those of the western group less, and those of the south-western, again, greater. These deviations may, in part, arise from the inexactness of the assumption with which we set out in the computation of the lines, and from the sensible deviation of those lines from parallelism. But they are probably owing in a much greater degree to the disturbing causes to which we have already alluded. The separate results composing each of these groups were for the most part obtained about the same time, and they are therefore probably affected in the same manner, and nearly in the same amount, by the irregular fluctuations in the direction and intensity of the resultant magnetic force. Of these, the changes in the *direction* of the force are by far the most influential. The relation between the corresponding changes in the dip and in the horizontal intensity is expressed by the formula

$$\frac{dh}{h} = - \tan \delta \sin 1' \, d\delta ;$$

$d\delta$ being expressed in minutes. Hence when $\delta = 71^\circ 0'$, the change of the horizontal intensity, $\frac{dh}{h}$, corresponding to a change of *one minute* of dip is $-$ ·00084 ; and for a variation of 12′ in the dip, the corresponding variation of the horizontal force is ·01.

In deducing the lines of dip from observation, it seems advisable to separate the results of the two years. For the weights we shall assume

Limerick (1834) . . . weight = 5,
Armagh —— . . . —— = 2 ;

the weights of each of the other determinations being unity.

Making the computations for the year 1834, we obtain the following results :

| Place. | $(wa^2)$ | $(w\,a\,b)$ | $(wb^2)$ | $(w\,a\,c)$* | $(w\,b\,c)$* |
|---|---|---|---|---|---|
| Carn ....... | 1156 | − 3876 | 12996 | +1870 | −6270 |
| Strabane .... | 1764 | − 3696 | 7744 | +2268 | −4752 |
| Enniskillen . | 2304 | − 2880 | 3600 | +2592 | −3240 |
| Armagh .... | 392 | − 1680 | 7200 | +1008 | −4320 |
| Carlingford . | 9 | + 123 | 1681 | − 72 | − 986 |
| lla | 7744 | + 2464 | 784 | +1584 | + 504 |
| Limerick .. | 36125 | +17425 | 8405 | +2125 | +1025 |
| Killarney .. | 14641 | + 9438 | 6084 | + 786 | + 507 |
| Fermoy .. | 7396 | + 6880 | 6400 | −1548 | −1440 |
| Glengariff | 15129 | +11931 | 9409 | + 430 | + 339 |

Adding, we have,

$$S\,(w\,a^2) = 86660, \quad S\,(w\,a\,b) = +\,36129, \quad S\,(w\,b^2) = 64303,$$
$$S\,(w\,a\,c) = +\,11043, \quad S\,(w\,b\,c) = -\,18633.$$

The final equations accordingly are

$$86660\,x + 36129\,y = +\,11043$$
$$36129\,x + 64303\,y = -\,18633\,;$$

from which we deduce

$$x = +\,{\cdot}3228 = s\cos v,$$
$$y = -\,{\cdot}4705 = s\sin v\,;$$

in which $v$ denotes the angle which the isoclinal line makes with the meridian of Dublin, and $s$ the coefficient which determines the rate of increase of the dip in the perpendicular direction. Dividing, and squaring and adding, we find

$$\tan v = -\,1{\cdot}458, \qquad v = -\,55^\circ\,33',$$
$$s = {\cdot}571.$$

The following are the results of calculation for the year 1835:

* The values of $\delta - \delta_{\prime}$ or $c$, are only taken to the nearest minute.

| Place. | $(a^2)$* | $(a\,b)$ | $(b^2)$ | $(a\,c)$ | $(b\,c)$ |
|---|---|---|---|---|---|
| Belmullet ...... | 15625 | − 7375 | 3481 | +8562 | −4041 |
| Markree ......... | 5929 | − 4081 | 2809 | +4774 | −3286 |
| Ballina........... | 9604 | − 4802 | 2401 | +6468 | −3234 |
| Achill Ferry ... | 16129 | − 4953 | 1521 | +7816 | −2398 |
| Galway ......... | 8649 | + 372 | 16 | +2604 | + 112 |
| Rathdrum ...... | 4 | − 52 | 676 | + 23 | − 299 |
| Tulla ............ | 7744 | + 2464 | 784 | +2112 | + 672 |
| Ennis ............ | 9216 | + 2784 | 841 | + 768 | + 232 |
| Limerick ......... | 7225 | + 3485 | 1681 | − 170 | − 82 |
| Gorey ............ | 0 | + 0 | 1600 | + 0 | − 360 |
| Ballybunan ...... | 14641 | + 5566 | 2116 | +1936 | + 736 |
| Broadway ...... | 9 | + 201 | 4489 | − 87 | −1943 |
| Waterford ...... | 961 | + 2139 | 4761 | − 449 | −1000 |
| Dingle ............ | 22500 | +11250 | 5625 | + 750 | + 375 |
| Valentia ......... | 21316 | +12410 | 7225 | + 292 | + 170 |
| Cork ............ | 6889 | + 7221 | 7569 | −1826 | −1914 |
| Youghal ......... | 3481 | + 5192 | 7744 | −1416 | −2112 |

Summing, we find,

$$S(a^2) = 149922, \quad S(a\,b) = +\,31821, \quad S(b^2) = 55339,$$
$$S(a\,c) = +\,32157, \quad S(b\,c) = -\,18372;$$

so that the final equations are

$$149922\,x + 31821\,y = +\,32157,$$
$$31821\,x + 55339\,y = -\,18372.$$

From these we deduce

$$x = +\,{\cdot}3250, \quad y = -\,{\cdot}5196,$$
$$\tan v = -\,1{\cdot}599, \quad v = -\,57^\circ\,59',$$
$$s = {\cdot}613.$$

It would appear, then, that the angle which the isoclinal lines in Ireland make with the meridian is on the increase; a result which is in conformity with the general progress of these lines, as inferred from a comparison of recent observations with those of an earlier date.

For the mean of the two years,

$$x = +\,{\cdot}3239, \quad y = -\,{\cdot}4950,$$
$$\tan v = -\,1{\cdot}528, \quad v = -\,56^\circ\,48',$$
$$s = {\cdot}592.$$

The lines in the annexed chart are deduced from these last results; and it appears from them that the interval of the

* We have assumed that $w = 1$, for all the results obtained in this year.

lines, corresponding to a difference of half a degree of dip, is 50·7 geographical miles.

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The lines of dip and of horizontal intensity being known, the lines of total intensity may be deduced. For if $f$ denote the total intensity, $h$ its horizontal component, and $\delta$ the dip, as before,

$$h = f \cos \delta;$$

and differentiating, and dividing by the equation itself,

$$f^{-1}\, d f = h^{-1}\, d h + \tan \delta \sin 1'\, d \delta. \qquad \text{(I.)}$$

Now, if the values of $x$ and $y$ for the lines of dip and of horizontal intensity be denoted by $x_{(\delta)}$, $x_{(h)}$ and $y_{(\delta)}$, $y_{(h)}$, and if $x_{(f)}$ and $y_{(f)}$ be the corresponding quantities for the lines of total intensity,

$$\left.\begin{aligned} d\delta &= a\, x_{(\delta)} - b\, y_{(\delta)} \\ d h &= a\, x_{(h)} - b\, y_{(h)} \\ d f &= a\, x_{(f)} - b\, y_{(f)}, \end{aligned}\right\} \qquad \text{(II.)}$$

in which $a = (\mu - \mu_{\prime}) \cos \lambda$, $b = \lambda - \lambda_{\prime}$ (E.); $\mu$ and $\lambda$ being the longitude and latitude of *any* assumed station, and $\mu_{\prime}$ and $\lambda_{\prime}$ those of Dublin. Substituting these values in (I.), it becomes

$$\left.\begin{aligned} f^{-1}\left(a\, x_{(f)} - b\, y_{(f)}\right) &= h^{-1}\left(a\, x_{(h)} - b\, y_{(h)}\right) \\ &\quad + \tan \delta \sin 1' \left(a\, x_{(\delta)} - b\, y_{(\delta)}\right). \end{aligned}\right\} \qquad \text{(III.)}$$

But as $a$ and $b$ are entirely independent, their coefficients must be, separately, equal, and we have

$$\left.\begin{aligned} f^{-1}\, x_{(f)} &= h^{-1}\, x_{(h)} + \tan \delta \sin 1'\, x_{(\delta)} \\ f^{-1}\, y_{(f)} &= h^{-1}\, y_{(h)} + \tan \delta \sin 1'\, y_{(\delta)}, \end{aligned}\right\} \qquad \text{(IV.)}$$

so that the values of $x_{(f)}$ $y_{(f)}$ are found when those of $x_{(h)}$ $x_{(\delta)}$ $y_{(h)}$ $y_{(\delta)}$ are known.

Let the second members of the preceding equations (IV.) be denoted, for abridgement, by P and Q, then

$$x_{(f)} = t \cos w = f\,\mathrm{P},$$
$$y_{(f)} = t \sin w = f\,\mathrm{Q};$$

in which $w$ is the inclination of the line of total intensity to

the meridian, and $t$ the coefficient which determines the rate of increase. Dividing the latter by the former, there is

$$\tan w = \frac{Q}{P}, \qquad \text{(V.)}$$

and, squaring and adding,

$$t = f \sqrt{P^2 + Q^2}. \qquad \text{(VI.)}$$

From the preceding formulæ it appears that the direction of the isodynamic line at any point is dependent on the values of $h$ and of $\delta$ at that point, so that these lines will not be parallel, even though the lines of dip and of horizontal intensity should be so. The deviations, however, will not be considerable within the limits of Ireland; and for our present purpose it will be enough to seek the *mean* direction of the lines, and the *mean* rate of increase in the direction perpendicular to them. We must therefore employ in the preceding formulæ the values of $f$, $h$, and $\delta$, corresponding to the mean point of the island, or the point whose latitude and longitude are 53° 25′ and 7° 55′ *, and for which therefore

$$\lambda - \lambda_{\prime} = 4', \quad \mu - \mu_{\prime} = 100'.$$

Now it has been already found that

$$x_{(h)} = -\cdot 0001633, \quad x_{(\delta)} = +\cdot 3239,$$
$$y_{(h)} = +\cdot 0003748, \quad y_{(\delta)} = -\cdot 4950;$$

and substituting these values in the formulæ

$$\delta - \delta_{\prime} = (\mu - \mu_{\prime}) \cos \lambda \, x_{(\delta)} - (\lambda - \lambda_{\prime}) \, y_{(\delta)},$$
$$h - h_{\prime} = (\mu - \mu_{\prime}) \cos \lambda \, x_{(h)} - (\lambda - \lambda_{\prime}) \, y_{(h)};$$

we find $\delta - \delta_{\prime} = 21'\cdot 4$, $h - h_{\prime} = -\cdot 0113$. Consequently,

$$\delta = 71° \, 24'\cdot 4, \quad h = \cdot 9282, \quad \text{and } f = 1\cdot 0295.$$

We have now the numerical values of all the quantities which enter the formulæ

$$P = h^{-1} \, x_{(h)} + \tan \delta \sin 1' \, x_{(\delta)},$$
$$Q = h^{-1} \, y_{(h)} + \tan \delta \sin 1' \, y_{(\delta)};$$

and we find on substitution,

$$P = +\cdot 0001042, \quad Q = -\cdot 0000242.$$

Introducing these values in (V.) and (VI.),

$$\tan w = -\cdot 2322, \quad w = -13° \, 4', \quad t = \cdot 0001102.$$

* This point corresponds, almost exactly, to the town of Athlone.

These results, however, are not entitled to much confidence. An attentive consideration of the formulæ (IV.) and (V.) will show that the direction of the resultant isodynamic lines will vary very widely with moderate variations in the values of $x_{(h)}$ $y_{(h)}$ $x_{(\delta)}$ $y_{(\delta)}$ on which it depends; or, in other words, that a small error in the position of the lines of dip or of horizontal intensity will entail a very great one in that of the lines of total force. Thus, if we were to take for the lines of dip those inferred from the observations of the year 1835 alone, we should find

$$P = + \cdot 0001051, \quad Q = - \cdot 0000455,$$

$$\tan w = - \cdot 4329, \qquad w = - 23^\circ\ 25';$$

a result differing by more than 10° from the former. In these latitudes, therefore, very great accuracy is necessary in the determination of the lines of dip and of horizontal force before we can make, in this manner, even an approximation to the direction of the lines of total force. For these reasons the results of the direct method, to which we now proceed, seem to be deserving of more confidence.

In the calculation of the isodynamic lines from the results of observation by the statical method, we shall take the number of observations at each station to represent the weight of the result; we have in this manner

| | | |
|---|---|---|
| Limerick | . . . | weight = 4, |
| Armagh | . . . . | —— = 2, |
| Youghal | . . . | —— = 2; |

the weight of each of the other determinations being unity.

The following are the elements of the computation:

| Place. | $(w\,a^2)$ | $(w\,a\,b)$ | $(w\,b^2)$ | $(w\,a\,c)$ | $(w\,b\,c)$ |
|---|---|---|---|---|---|
| Carn ............ | 1156 | − 3876 | 12996 | + ·5236 | −1·7566 |
| Strabane ......... | 1764 | − 3696 | 7744 | + ·4284 | − ·8976 |
| Armagh ......... | 392 | − 1680 | 7200 | + ·1260 | − ·5400 |
| Belmullet ...... | 15625 | − 7375 | 3481 | +1·1875 | − ·5605 |
| Markree ......... | 5929 | − 4081 | 2809 | + ·7161 | − ·4929 |
| Ballina............ | 9604 | − 4802 | 2401 | + ·7742 | − ·3871 |
| Achill Ferry...... | 16129 | − 4953 | 1521 | +1·2446 | − ·3822 |
| Galway ......... | 8649 | + 372 | 16 | + ·8184 | + ·0352 |
| Rathdrum ...... | 4 | − 52 | 676 | + ·0114 | − ·1482 |
| Tulla ............ | 7744 | + 2464 | 784 | + ·6776 | + ·2516 |
| Ennis ............ | 9216 | + 2784 | 841 | + ·5376 | + ·1624 |
| Limerick ......... | 28900 | +13940 | 6724 | +1·3940 | + ·6724 |
| Gorey ............ | 0 | + 0 | 1600 | + ·0000 | − ·2720 |
| Ballybunan ...... | 14641 | + 5566 | 2116 | +1·5367 | + ·5842 |
| Broadway ...... | 9 | + 201 | 4489 | − ·0075 | − .1675 |
| Waterford ...... | 961 | + 2139 | 4761 | − ·1085 | − ·2415 |
| Dingle............ | 22500 | +11250 | 5625 | +2·1000 | +1·0500 |
| Valentia ......... | 21316 | +12410 | 7225 | +1·2994 | + ·7565 |
| Cork ............ | 6889 | + 7221 | 7569 | − ·0664 | − ·0696 |
| Youghal ......... | 6962 | +10384 | 15488 | + ·1298 | + ·1936 |

By addition we obtain

$$S\,(w\,a^2) = 178390, \quad S\,(w\,a\,b) = +\,38216, \quad S\,(w\,b^2) = 96066,$$
$$S\,(w\,a\,c) = +\,13{\cdot}3229, \quad S\,(w\,b\,c) = -\,2{\cdot}2448.$$

The final equations therefore are

$$178390\,x + 38216\,y = +\,13{\cdot}3229,$$
$$38216\,x + 96066\,y = -\,\;\;2{\cdot}2448;$$

from which we obtain, by elimination,

$$x = +{\cdot}00008711, \quad y = -\,{\cdot}00005802.$$

Consequently,

$$\tan w = -\,{\cdot}6661, \quad w = -\,33^\circ\,40', \quad t = {\cdot}0001047.$$

The lines of total intensity thus deduced are laid down in the annexed chart for differences of ·005, these differences corresponding to intervals of 47·6 geographical miles. It will be seen that their direction diverges widely from that of the lines of dip; and although the position of the two classes of lines may need further correction, it does not seem likely that such correction will have the effect of diminishing, at least by any considerable amount, the divergence.

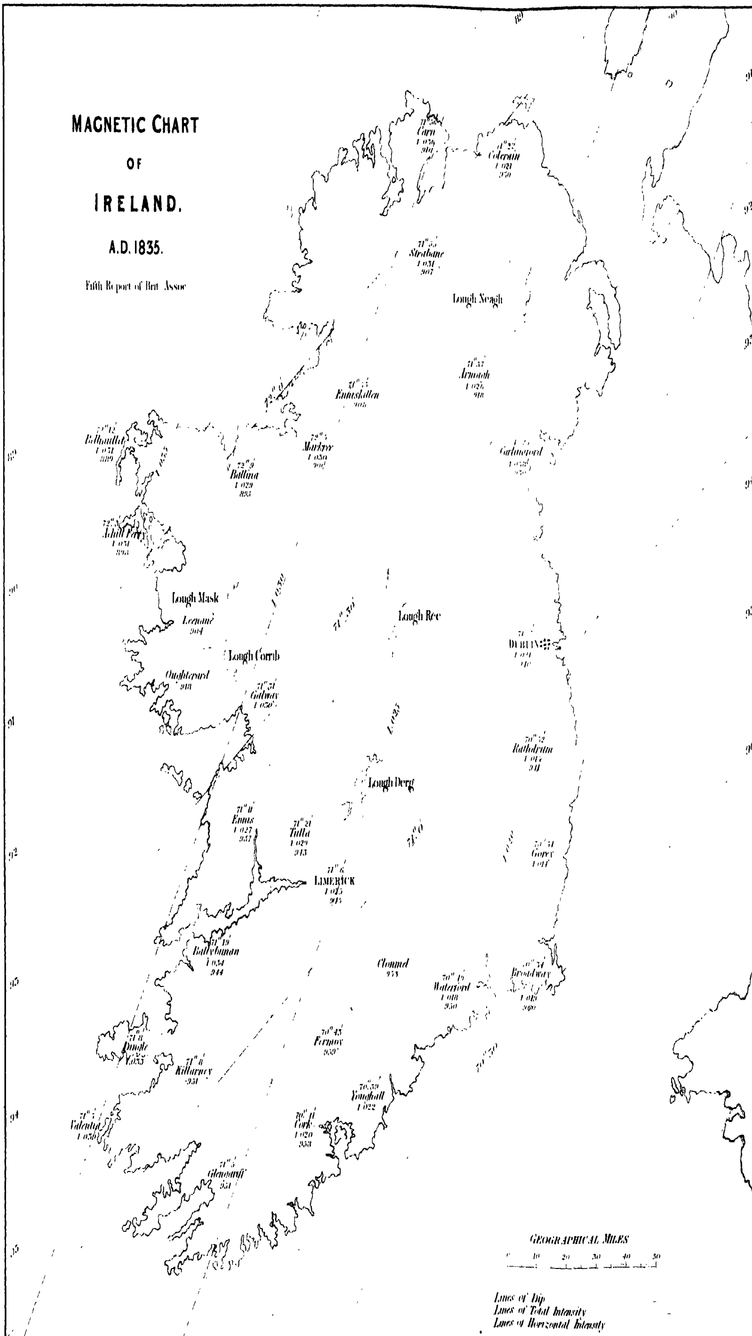
# MAGNETIC CHA

OF

# IRELAND

A.D. 1835.

Fifth Report of Brit. Ass


MAGNETIC CHART
OF
IRELAND.
A.D. 1835.
Fifth Report of Brit. Assoc.
Carn
Coleraine
Strabane
Lough Neagh
Armagh
Enniskillen
Markree
Ballina
Carlingford
Achill Ferry
Lough Mask
Leenane
Lough Ree
Lough Corrib
DUBLIN
Oughterard
Galway
Rathdrum
Lough Derg
Ennis
Tulla
Gorey
LIMERICK
Ballybunian
Clonmel
Waterford
Broadway
Fermoy
Dingle
Killarney
Youghall
Valentia
Cork
Glengariff
GEOGRAPHICAL MILES
0 10 20 30 40 50
Lines of Dip
Lines of Total Intensity
Lines of Horizontal Intensity

## *On the Phænomena usually referred to the Radiation of Heat.* *By* HENRY HUDSON, *M.D., M.R.I.A. Dublin.*

THE following paper contains the results of a portion of a series of experiments planned several years since, with a view to an experimental analysis of the phænomena ascribed to the radiation of heat. The apparatus generally employed consisted (as in Leslie's experiments) of cubic tin canisters and differential thermometers, together with a parabolic zinc mirror of $17\frac{1}{2}$ inches diameter and $4\frac{1}{4}$ inches concavity; this was made with a hollow back to it, and a short projecting pipe at the top for the purpose of filling it with any hot or cold liquid at pleasure. I had also an apparatus for heating or cooling the balls of the differential thermometer previously to arranging the instrument in its proper position before the mirror; my object being to examine the different effects produced on the focal ball under all possible combinations of varying the temperatures of the canister, the mirror, and the thermometer.

Having found Leslie's differential thermometer (containing sulphuric acid) to be not sufficiently sensitive where the variations of the temperature were small, I made a differential thermometer for these purposes, into which I introduced sulphuric æther coloured with dragon's blood; I shall therefore speak of this instrument as the "ætherial thermometer," to distinguish it from the common differential thermometer.

Having observed, in previous experiments, that the radiating power of a surface covered with black japan varnish was about twelve times greater than that of a metallic surface, I have in the experiments to be detailed merely made use of these two kinds of surfaces, my principal object being to ascertain the nature of *radiation*, a term which I beg leave to use, whether with regard to heat or cold, without thereby intending to imply any reference to the theory by which the phænomena are to be accounted for. Before proceeding to the more immediate objects of my experiments I may state that I have found Professor Leslie's conclusions on the three following points fully confirmed, viz. 1st, If the canister, the mirror, and the thermometer be all of the same temperature with the air, the focal ball is not affected either by the metallic or the varnished side of the canister: 2nd, If the canister *alone* be heated, the focal ball is more warmed by the varnished than by the metallic side in the proportion of

about 12 to 1: and 3rd, If the canister *alone* be cooled, the focal ball is more chilled by the varnished than by the metallic side in the *same* proportion of about 12 to 1.

I now proceed to give a few examples of such experiments as have been sufficiently frequently repeated to remove all doubt, from my own mind, of their having been produced by occasional or accidental causes.

*Experiment* 1.—In a close room (temperature 62° Fahrenheit) I placed a large tin screen, 4 feet 6 inches by 2 feet 9 inches, in front of the mirror, at a distance of about 6 feet; and having ascertained the position of the focus, I filled the hollow back of the mirror with water at 200° of Fahrenheit, and arranged the ætherial thermometer so that one of the balls being in the focus the other was equally heated by the mirror, the instrument marking zero. I now placed a smaller tin screen, 24 inches by 17, varnished on one side, about a foot in front of the large screen, with its metallic surface facing the mirror: on so doing the focal ball was chilled *above* one fourth of a degree; and on turning the varnished side towards the mirror the focal ball was cooled $3\frac{1}{2}$ degrees below zero. On moving the small screen nearer to the mirror, the chilling effects of both sides increased, and more rapidly than they should have done in reference to the mere diminution of the distance; a fact indeed which may be inferred from the effect of the *metallic* side of the small screen in its primary position. I also remarked that when either side of the screen was left facing the mirror for any considerable time, its effect *began* to diminish, evidently from that surface becoming warmer. But the other side being then turned produced its peculiar effect; and the former side also being again (after the lapse of a few minutes) put fronting the mirror was found to produce its *full* effect as at first.

*Experiment* 2.—The large screen and the thermometer being arranged as before, and the mirror heated to 173°, I substituted for the smaller screen a ten-inch canister filled with water at 59°, the temperature of the room being 55° only; and on repeating the trials, as in the previous experiment, I found that this also acted as a *cold* body, and the varnished side produced the greater effect: just in front of the large screen, however, its effects became very small, the black side only producing a chilling effect of about three fourths of a degree. I then filled the canister with water at 67°, and (in this same position) it now acted as a warm body, and the varnished side most efficaciously. On moving it gradually nearer to the mirror, the effects diminished, and at length altogether ceased, so that the thermometer remained at zero, whether the metallic or the varnished side of the mirror

was towards it. On moving the canister still nearer to the mirror, it now began to act as a *cold body*, and the varnished side, as before, showed its superior efficiency.

The same experiments were also tried (and with the same effects), by interposing screens between the balls of the thermometer and the canister, lest the *direct* radiation to the two balls of the thermometer might not have been equal, which would have confused the results.

*Experiment* 3.—Having covered the balls of the ætherial thermometer with cambric, and placed one of the balls in the focus of the mirror (of the same temperature as the air = 48° of Fahrenheit), I applied water to the focal ball, and the chilling effect from the evaporation was equal to 20°. On placing the screen before the mirror there was no difference in the effect, whether the metallic or the varnished side of the screen faced the mirror.

*Experiment* 4. I now covered the balls of a common differential thermometer with cambric, and having arranged one ball in the focus, as in the last experiment, I applied *æther* with a camel's hair pencil to the focal ball; the temperature of the room being 48°, the evaporation of the æther chilled the ball 102°, as the *extreme* effect, and there was still no difference whatever in the effect, whether the varnished or the metallic side of the screen was opposed to the mirror. In other similar experiments (the temperature of the room being higher,) the evaporation of æther chilled the ball beyond the extremity of the scale of the instrument; I therefore altered the zero, making the liquid stand a considerable space below the commencement of the scale. The temperature of the room being 51°, I found a common mercurial thermometer covered with cambric sunk to 24° from the evaporation of the æther, and, at the same time, the evaporation from the ball of the differential thermometer made *it* stand at 62¾° on its own scale. There was no difference, as already observed, in the effect produced by either side of the screen. I then filled the canister with a mixture of ice and water, (its temperature was 35°,) and on placing the varnished side of the canister opposite to the mirror, the evaporation of the æther now cooled the thermometer to 64½°, being 1¾° more than the previous chilling effect, so that the canister (though *warmer* than the focal ball) acted on it as if it were a colder body. It is to be observed also that the *direct* chilling effect of the canister must be supposed to be, at all events, *greater* on the non-focal ball from the consideration of their respective temperatures, and that, consequently, the result ought, on this supposition, to have been precisely the reverse of what has been mentioned.

The evaporation of the æther was so rapid that I found these experiments very troublesome, and in order to obtain the maximum effect I found it necessary to have the æther previously *cooled* considerably; thus, in the last experiment, even when the æther was cooled to 34° of Fahrenheit, on dropping some of it on the ball, the *immediate* effect was to heat the focal ball from 3 to 8 or 10 degrees, according to the quantity of æther dropped on it at the same time.

Being desirous to try the "diathermancy" (to use Melloni's term) of rock salt, I had a plate sawed out of the largest block of it I could procure at the time: its dimensions were 8 inches by 6, and four tenths of an inch thick. I placed a screen with an opening in it (which the plate of salt just filled up) at 2 feet 9 inches from the mirror, having one ball of the ætherial thermometer in the focus, and placed the 10-inch canister containing hot water with its varnished side 2 feet behind the screen. While the rock salt remained in the opening the thermometer continued at zero, but on removing it the thermometer immediately began to rise, and in less than ten minutes had risen 14°. On replacing the rock salt the thermometer fell again, and in a few minutes sunk to zero.

The plate of salt was certainly not a favourable specimen, and therefore I should not lay much stress on the present experiment if Melloni's expressions had not led me to expect a different result from even the worst specimens of this substance.

It is unnecessary to point out how completely the experiments I have mentioned are at variance with the received doctrine of the radiation of heat, as, on such a theory, the radiation of cold appears to be equally established. If the mirror be heated or cooled, and the thermometer so placed that both balls are equally heated or cooled by the mirror, there is no reason why any heating or cooling radiation towards the mirror should affect the focal ball; if we suppose such radiation to heat or cool the mirror, both balls (from their position) should be equally affected. Now we *might* suppose heat to be merely radiated to and reflected by the mirror, but we cannot admit the same with regard to cold. Why then does this *appear* to be the case, both balls being equally affected by the temperature of the mirror; why does the focal ball appear to radiate more heat *towards the mirror* than the non-focal ball when a (comparatively) cold body is placed opposite to the mirror?

On the other hand, these experiments appear to be in no respect incompatible with the views of the late Professor Leslie, whose ingenious theory (whether true or false) has not, in my mind, ever received the attention to which I think it is entitled.

In considering the air as the medium of the transfer of heat, he supposes that the hot or cold surface of the canister heats or cools, and consequently causes an expansion or contraction of, the adjoining atmosphere;—that the first layer of air thus expanded or contracted presses on or is compressed by the portion before it;—that this *process is renewed in a rapid succession,* and that an undulatory motion of the heated or chilled air is thus propagated to the mirror and thence reflected to the focus; and each pulsation being accompanied by a discharge of heat from the portion of air at the higher temperature, that the heating or cooling effect is conveyed to the thermometer simultaneously with the progress of the undulation. I may here remark that it appears not improbable that such expansions and contractions should take place; and when we take into account the change in the capacity of air for heat produced by expansion or compression, we have at least a plausible reason for the *transfer* of heat which he supposes to accompany the aerial undulation.

I do not wish to be understood as adopting Leslie's views, but I conceive them deserving of further investigation, and the only conclusion I wish at present to draw from the previous experiments is, that they are only explicablé on *an* undulatory theory, and consequently, if air be not the medium of the transfer, that they furnish an additional and perhaps conclusive argument in favour of the undulatory theory of light.

I believe no cause has been attempted to be assigned for the difference in the radiating powers of surfaces except by Leslie, who supposes it to arise from the different distances of the atmospheric boundary; I may, perhaps, therefore, be allowed to refer to a known property of bodies which probably ought to lead us to anticipate such results, viz. their different *capacities for heat.* If two surfaces* are of the same elevated temperature and placed in the same medium, they may be considered as having the *same* tendency to attain the temperature of that medium, and may consequently be expected to give off the *same portion* of their excess of *temperature,* and consequently quantities of heat *proportional* to their capacities. If we look to classes of bodies I believe they will be found to be in accordance with the cause I have just ventured to assign.

Having recently received one of Melloni's thermomultipliers, I have made a few experiments with reference to the transmission of heat (from boiling water) through crystals (of rock salt, alum, and rock crystal), which were sent with the instrument. I found the diathermancy of rock salt very marked, though not

* Taking the term in the *physical* sense of having some definite thickness, which may be different in different substances.

at all so high as Melloni states it to be: thus, taking the effect without a screen as $= 9\frac{1}{2}^\circ$, I found it

with rock salt $= 6\frac{1}{2}^\circ$,
with rock crystal $= 1\frac{1}{2}^\circ$,
and with alum . . $= 1^\circ$.

Having removed the warm canister entirely out of the axis of the pile, so that the needle stood at zero, I then successively put the rock salt, the rock crystal, and the alum to the opening in the screen, and in one set of experiments I found the following results:

with rock salt, needle marked *about* $1\frac{1}{4}^\circ$,
with rock crystal, —— *above* $\frac{1}{2}^\circ$,
with alum, —— *less than* $\frac{1}{2}^\circ$,

In another trial, with warmer water, the results were

with rock salt . . . $2\frac{1}{4}^\circ$,
with rock crystal *above* $1^\circ$,
with alum . . *nearly* $1^\circ$.

I may add that the effect on the needle appeared instantaneous. These experiments are, confessedly, imperfect, and I only mention them for the purpose of pointing out a simple mode of answering the third and fourth questiôns on radiant heat contained in the second volume of the *Reports of the Association*, as to "whether heat is *transmitted* through certain substances like light, or whether it is merely rapidly communicated by *conduction*, &c.*" If (as there may perhaps be some reason to suspect, even from these imperfect trials,) there be no *direct* transmission of simple heat, we may expect to find the same effects produced by a given source of heat whether it be in or out of the axis of the thermoscope, *provided* the crystal and the canister be so arranged that they are at the same distance from each other and the inclination of their surfaces alike in each case. I may mention also, as bearing on this point, that with the canister in the axis (its position, surface, and temperature being the same,) the effects on the crystals increased with the *extent* of the radiating surface, evidently from the crystals being *more warmed*, but that there was, nevertheless, no apparent change produced in the *ratios* of the effects with the different sorts of crystals. I may also refer to Melloni's own remark with regard to the effect of increased thickness in the screen, "that the obstruction is not at the first surface (as with light), but (as if the heat were *conducted* through the screen) in the substance of the screen itself."

* On this subject see *Lond. and Edinb. Phil. Mag.*, vol. viii. p. 109.

With reference to the thermomultiplier, it will, I believe, be found to be *most* useful in experiments with screens of various sorts; its delicacy is, no doubt, very great, but its expensive nature is unfavourable to its general introduction. I believe the ætherial thermometer will be found *at least* as delicate an *indicator* of small changes of temperature; but the instrument may be made still more sensitive by using æther saturated with ammonia, or incomparably more delicate still by substituting other liquids, as Faraday's "volatile carburetted hydrogen," or the liquids of the *condensed gases*, for instance, sulphurous acid or (what I should prefer) euchlorine. It may be thought absurd to suggest such an application of these latter bodies, but I can vouch for its practicability, at least with reference to sulphurous acid, as I actually produced the effect by introducing quicksilver and sulphuric acid into one of the balls of a differential thermometer, and after it was sealed applying heat to the ball. Not being certain how far I had succeeded, I incautiously applied my hand to the other ball, on which the tube instantly broke where it had been joined, and the colourless liquid, which had previously nearly half filled this ball, entirely disappeared, with an overpowering evolution of sulphurous acid gas. I have not had time since to make another trial, but I hope to be able to accomplish it in the course of the next month.

Should the experiments mentioned in the beginning of this paper be thought to throw any light on a difficult subject, I feel it but justice to state, in concluding, that any advantage which science may derive from their execution is essentially due to the spirited individuals who originated the British Association, as though forming a portion only of a series planned many years ago, I should probably have never carried them into effect if it had not been for the stimulus produced by reading Professor Powell's excellent Report in the first volume of the *Transactions* of the Association.

# *Third Report of Experiments on the Quantities of Rain falling at different Elevations above the Surface of the Ground at York, undertaken at the request of the Association by* WILLIAM GRAY, *Jun., and Professor* PHILLIPS, *F.R.S. F.G.S., Secretaries of the Yorkshire Philosophical Society; with Remarks on the Results of the Experiments, by Professor* PHILLIPS.

AT the conclusion of the second series of experiments, the square gauges, which had been employed for two years, were removed, and replaced by others of a different form, which were arranged in a different manner. Three gauges were placed at each station, and a duplicate set of the ground gauges was fixed in Mr. Phillips's garden. Each gauge was cylindrical, 5 inches in diameter and 12 inches high; one of them (C) was an open cylinder, the others (B and A) were furnished with a funnel decreasing to a hole of $\frac{1}{4}$ inch, and a small lateral discharge-pipe $\frac{1}{4}$ inch in diameter, but 1 inch below the edge. This discharge-pipe was left constantly open. The object of this arrangement was to procure data as to the rate of evaporation at the different stations, both from the open vessel C and the gauges A and B. For this purpose a certain depth of water was poured into C, and its level, fluctuating with evaporation and rain, was measured at the same time that the gauges A and B were examined. The difference in inches and tenths between the measure of the water first introduced into C, augmented by the depth of rain in A,—and the depth of the residuary water in C,—gave the amount of evaporation in a given time.

The difference between A and B was this: The gauge A was emptied frequently, sometimes immediately after rain, while B was left for longer periods. The difference of the measured rain in each gave of course the difference of the evaporation from them. This part of the experiments served only to prove that the amount of evaporation from either gauge was very small. From various causes, and principally from the extreme inconvenience attending the laborious ascent of the Minster and Museum, the experiments on evaporation were not persisted in after August.

The following is a general table of results for the third year

| 1834-35. | RAIN. | | | EVAPORATION from Feb. 1. to the several dates. | | |
|---|---|---|---|---|---|---|
| | Minster. | Museum. | Ground. | Minster. | Museum. | Ground. |
| | Inches. | Inches. | Inches. | Inches. | Inches. | Inches. |
| Feb. 1 to March 1. | 0·180 | 0·670 | 1·040 | 2·330 | 1·220 | 0·610 |
| 6. | 0·416 | 0·600 | 0·772 | 3·096 | 1·570 | 2·312 |
| 21. | 0·010 | 0·110 | 0·262 | 4·936 | 2·480 | 2·921 |
| April . . . 12. | 0·193 | 0·326 | 0·558 | 7·129 | 4·256 | 3·782 |
| 21. | —— | —— | —— | 8·229 | 4·816 | 4·432 |
| May . . . 1. | 0·810 | 0·982 | 1·115 | 8·889 | 5·522 | 4·667 |
| 16. | 0·219 | 0·300 | 0·360 | 11·068 | 6·738 | 5·482 |
| June . . . 18. | 1·080 | 1·726 | 1·862 | 14·683 | 9·611 | 7·169 |
| July . . . 11. | 0 021 | 0·115 | 0·325 | 18·159 | 10·829 | 9·691 |
| 21. | 1·930 | 2·770 | 3·240 | 19·539 | 12·019 | 11·131 |
| August . . . 9. | 0·173 | 0·360 | 0·540 | 22·212 | 14·559 | 11 674 |
| 30. | 0·720 | 0·940 | 1·220 | 23·707 | 16·519 | 12·691 |
| October . . 3. | 1·127 | 1·526 | 1·815 | —— | —— | —— |
| January . . 31. | 1·085 | 1·710 | 2·830 | —— | —— | —— |
| Totals . . | 8·294 | 12·135 | 15·939 | | | |
| Ratios . . | 52·03 | 76·13 | 100·00 | | | |

| | QUANTITIES OF RAIN. | | |
|---|---|---|---|
| | Minster. | Museum. | Ground. |
| | Inches. | Inches. | Inches. |
| 3 warm months .... | 3·924 | 5·911 | 7·187 |
| 5 warm months .... | 5·270 | 7·737 | 9·362 |
| 7 warm months ... | 6·273 | 9·015 | 11·035 |
| 7 cold months ...... | 2·314 | 3·416 | 5·462 |
| 5 cold months ...... | 2·021 | 3·090 | 4·901 |
| 3 cold months ...... | 1·565 | 2·380 | 3·870 |

## *Remarks on the Results of the Experiments.*

The results in the preceding table complete the series of three years' observations which it was originally proposed to execute; the gauges have now been removed, and the experiments are ended. A condensed view of the conclusions which seem fairly derivable from them may be useful, as a preliminary to the mathematical investigations and further experimental researches which the subject appears to demand.

No sooner was the first series of results tabulated, than they were easily seen to be principally dependent on two ascertainable

conditions, viz., the vertical measure of the tract of air intervening between the stations, and the temperature of the season of the year; the former determining the *ratio* of the differences of quantity of rain at different elevations above the ground, the latter influencing the *amount* of these differences. The dependence of this amount upon the temperature inversely, and consequently upon the humidity of the season directly, led to an attempt at a simple explanation of the phænomenon, not materially different from that proposed (as M. Arago has informed me) without experimental proof by M. Boisgiraud.

The second series of observations confirmed very completely the conclusion previously adopted, of the dependence of the *amount* of the difference of rain between a station on the ground and others at some height above it, upon the temperature inversely. But the *ratio* of the differences at different elevations, which had been formerly supposed constant, was found to vary, and also to exhibit some characteristic variations in different seasons of the year. This suggested the hope of determining, by a third series, the limits of the variation of the ratio, not only in different years, but in different seasons of the year, and of thus advancing a considerable step toward a satisfactory solution of the whole problem.

We may first consider the *amount* of the differences between the quantity of rain at the upper stations and that on the ground; this being exactly proportional to the coefficient $m$ in the discussion of the first series.

*Table of the Quantities of Rain and Ratios of the Quantities at the Three Stations, for Three Years, in different Seasons of the Year.*

| | Minster. | Museum. | Ground. | Ratios. | | |
|---|---|---|---|---|---|---|
| | Inches. | Inches. | Inches. | | | |
| 3 summer months.... | 13·473 | 17·430 | 20·306 | 66·35 | 85·83 | 100 |
| 5 warmer months.... | 20·012 | 26·126 | 30·916 | 64·82 | 84·50 | 100 |
| 7 warmer months.... | 24·834 | 32·320 | 38·551 | 64·42 | 83·81 | 100 |
| 7 colder months...... | 18·220 | 25·100 | 33·999 | 53·58 | 73·82 | 100 |
| 5 colder months...... | 14·130 | 19·789 | 26·879 | 52·60 | 73·62 | 100 |
| 3 winter months .... | 14·138 | 12·170 | 17·320 | 49·94 | 70·26 | 100 |
| General ratio derived from the sums of the quantities | | | | 59·15 | 79·11 | 100 |

*Table of the Differences between the Ratios of the Quantities of Rain at the Upper, Middle, and Lower Stations, in relation to the Temperature of the Season.*

| | Temp. $t'$ | Differences from 100. | | Sum of $d + d'$. | $\Delta \frac{t}{t'}$. |
|---|---|---|---|---|---|
| | | $d$ | $d'$ | | |
| | ° | | | | |
| 3 summer months.... | 60·8 | 33·65 | 14·17 | 47·82 | 48·12 |
| 5 warmer months .... | 58·5 | 35·18 | 15·50 | 50·68 | 50·00 |
| 7 warmer months.... | 55·1 | 35·58 | 16·16 | 51·74 | 52·93 |
| 7 colder months...... | 40·8 | 46·42 | 26·18 | 72·60 | 70·10 |
| 5 colder months...... | 39·3 | 47·40 | 26·38 | 73·78 | 74·47 |
| 3 winter months .... | 36·3 | 50·06 | 29·74 | 79·80 | 80·61 |
| Whole year .................. | 48·2 (= $t$) | | | 60·71 $\Delta$ | 60·71 |

The last column gives the numbers corresponding to the simple formula $\Delta \frac{t}{t'}$, which shows clearly the real dependence of the *amount* of the diminution of rain at the upper stations upon the temperature of the season inversely.

The dependence of the *ratio* of the differences of rain, at diffferent elevations above the ground, upon the height of the stations, will appear from the following tables:

*Table to show the Mean Annual Value of the Function of the Height ($\phi\, h$).*

| | Quantities. | | | Differences from 100. | | Resulting value of $\phi\, h$. |
|---|---|---|---|---|---|---|
| | | | | | $d'$. | |
| | Inches. | Inches. | Inches. | | | |
| First series | 15·715 | 20·182 | 23·785 | 33·90 | 14·70 | $h^{\cdot 53}$ |
| Second do. | 14·963 | 19·850 | 25·706 | 41·79 | 22·78 | $h^{\cdot 38}$ |
| Third do. .. | 8·294 | 12·135 | 15·939 | 47·97 | 23·87 | $h^{\cdot 44}$ |
| General mean of three years .... | 12·990 | 17·389 | 21·810 | 40·44 | 20·27 | $h^{\cdot 437}$ |
| Mean of the differences from 100 | | | | 41·22 | 20·45 | $h^{\cdot 443}$ |

*Table to show the Variation of $\phi\, h$ in different Seasons of the Year, according to Three Years' Observations.*

| | $t_{\prime}$. | $\phi\, h$ derived from observation. | $\phi\, h$ calculated $= \frac{t'}{110}$ |
|---|---|---|---|
| | Inches. | | |
| 3 summer months.... | 60·8 | $h^{\cdot 55}$ | $h^{\cdot 55}$ |
| 5 warmer months.... | 58·5 | $h^{\cdot 52}$ | $h^{\cdot 53}$ |
| 7 warmer months.... | 55·1 | $h^{\cdot 50}$ | $h^{\cdot 50}$ |
| 7 colder months...... | 40·8 | $h^{\cdot 37}$ | $h^{\cdot 37}$ |
| 5 colder months...... | 39·3 | $h^{\cdot 37}$ | $h^{\cdot 357}$ |
| 3 winter months .... | 36·3 | $h^{\cdot 33}$ | $h^{\cdot 33}$ |

It follows from the last table that the exponent of the function of height varies as the temperature of the season, or

$$\phi\, h = \frac{\cdot 44\, t'}{t} = \frac{t'}{110}$$

The whole may therefore be put under the form $d = p\, h^{\frac{t'}{110}}$ and the value of the coefficient $p$ may be determined from the following table:

*Table of the Value of $p$ considered variable with the Temperature, and Height given in Feet.*

| | Sum of $d + d'$. | Sum of the values of $\phi\, h$ & $\phi\, h'$ observed. | $p$ inferred. | $p$ calculated $= b\, \frac{t^{2\cdot 8}}{t'^{2\cdot 8}}$ |
|---|---|---|---|---|
| Yearly.................. | 60·71 | 15·84 | (*b*) 3·830 | |
| 3 summer months........ | 47·82 | 27·05 | 1·768 | 1·999 |
| 5 warmer months........ | 50·68 | 23·36 | 2·169 | 2·227 |
| 7 warmer months........ | 51·74 | 21·19 | 2·441 | 2·580 |
| 7 colder months ........ | 72·60 | 11·31 | 6·419 | 6·108 |
| 5 colder months ........ | 73·78 | 11·31 | 6·523 | 6·783 |
| 3 winter months ........ | 79·80 | 9·34 | 8·544 | 8·472 |

The last column shows how nearly the value of the coefficient $p$ is represented by a function of the temperature of the season. By using a constant the agreement becomes very close,

$$\left(p_{\prime} = 4\, \frac{t^{2\cdot 8}}{t'^{2.8}} - \cdot 200\right).$$

Having thus reduced both the function of height and the coefficient to a dependence on temperature, we are enabled to institute a very severe comparison of the formula with experiment.

| | Observed. | | Calculated by $\phi h = h \frac{t'}{110}$ and $p = b \frac{t^{2\cdot8}}{t'^{2\cdot8}}$. | | Calculated with $p' = 4 \frac{t^{2\cdot8}}{t'^{2\cdot8}} - \cdot200$. | | Values of | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | $d$. | $d'$. | | | | | $\phi h$, | $\phi h_{\prime}$ | $p$, | $p_{\prime}$ |
| Annual ....... | 40·44 | 20·27 | 40·48 | 20·18 | 40·16 | 20·02 | 10·57 | 5·27 | 3·83 | 3·80 |
| 3 summer months | 33·65 | 14·17 | 38·10 | 15·90 | 35·85 | 15·00 | 19·07 | 7·98 | 1·99 | 1·88 |
| 5 warm months . | 35·18 | 15·50 | 38·00 | 16·40 | 36·14 | 15·61 | 17·13 | 7·40 | 2·22 | 2·11 |
| 7 warm months . | 35·38 | 16·60 | 37·40 | 17·36 | 36·96 | 17·16 | 14·49 | 6·73 | 2·58 | 2·550 |
| 7 cold months . . | 46·42 | 26·18 | 44·34 | 24·65 | 44·8 | 24·96 | 7·26 | 4·04 | 6·10 | 6·179 |
| 5 coldest months | 47·40 | 26·38 | 45·70 | 25·90 | 46·37 | 26·35 | 6·74 | 3·83 | 6·78 | 6·88 |
| 3 winter months. | 50·06 | 29·74 | 49·38 | 30·30 | 50·37 | 29·89 | 5·83 | 3·46 | 8·47 | 8·64 |

It may be proper to offer some remarks on the probable consequences to meteorology of prosecuting this subject both mathematically and experimentally, and to indicate a form of experiment which may be advantageously followed in all cases. Admitting that the three years' results above discussed are a fair average for the climate of York, and adopting the formula as expressing at least the nature of the proximate influential causes, we shall find its interpretation full of curious interest.

First, it must follow that, upon the average, rain (commencing at what Sir John Herschel calls the vapour plane) originates at a greater elevation in the atmosphere in the summer than in the winter. This is in conformity with Crossthwaite's Table of Clouds, given in Dr. Dalton's *Meteorological Essays*, where in the five warmer months, May, June, July, August, and September, 219 clouds in a month are noticed above 1050 yards in height, but in November, December, January, February, and March only 126.

The same inference results from considering the rain as originating either at a height corresponding to a certain reduced temperature, or at the point in the air where the dew-point = the m.t. of the period. In this latter case, using Mr. Daniell's numbers for the climate of London, the vapour plane, or origin of rain, will be

In January .............. 900 feet high.
In July .................. 3270 ,,
For the whole year ... 1650 ,,

The view previously advanced, that the rain-drops augment

in passing downwards, is in strict conformity with these results*.

Secondly. It appears to indicate that the air very near the ground is much more highly charged with vapour than that at moderate heights, so that the moisture of the air would seem to follow a very different law of distribution from that of mean temperature, the dew-point approximating to or meeting the mean temperature on the ground, and at one or more variable heights above it. As far as a few observations can be relied on, this effect does obtain to the height of 25 feet in the day during summer.

Experiment July 9, 1835.—In a garden where rain had fallen last night. Three thermometers (A dry, B and C wetted). Cloudy day, but sun partially appearing; 10 A.M.

| | A. | δ. | B. | γ. | C. |
|---|---|---|---|---|---|
| 1 inch from ground, gravel walk | 62·5 | 2·6 | 59·9 | 3·5 | 59·0 |
| 30 inches above | 62·4<br>62·3 | 1·0 | 58·5<br>58·2 | 4·95 | 57·5<br>57·3 |
| 150 inches above | 63·5<br>63·3 | 4·7 | 58·9<br>58·5 | 5·55 | 57·9<br>57·8 |
| 270 inches | 64·1 | 5·6 | 58·5 | 6·6 | 57·5 |

On the same day I ascended the Minster with Mr. William Gray, and found with other thermometers,

| | A. | B. | δ. |
|---|---|---|---|
| 1 feet high before ascending | 61·5<br>61·5<br>61·4 | 56·7 | 4·8 |
| 80 feet high | 64·3<br>63·6 | 58·3<br>57·3 | 5·6 |
| 200 feet high (dew-point 52°) | 62·5<br>62·0 | 56·5<br>56·4 | 5·8 |
| Ground (2 inches above) after descending | 62·1 | 57·55 | 4·55 |

Rain fell immediately afterwards, while the dew-point = 52° and air 62°.

Thirdly. We may be sure that by the continuation of experiments in well-selected situations, the influence of the variation of climate,—both with relation to mean temperature, moisture, the general aspect and surface of a country,—may be determined. As far as can be seen at present, it appears probable that the

* Mr. Howard's view of an indefinite raining space is less applicable.

function of the height would be greater and the coefficient less, and that both of these would be subject to less variation in lower latitudes, and the contrary. It is also probable that the decrease in the *quantity of rain*, as we ascend above the surface, will be least in the warm regions of the globe and greatest in the cold zones. There is less difference at Paris than at York.

Fourthly. It is probable that in countries uniformly moist, or of uniform temperature, the results would be materially different from those obtained in a climate so changeable as that of the British Islands.

Fifthly. It would follow from the formula that the differences in the values of $\phi\, h$ and $p$ in different years depend very much upon the season of the year when the rain fell in greatest quantity. Thus, $\phi\, h$ was greatest in 1832–3, and nearly twice as much rain fell in the warm as in the cold months; $\phi\, h$ was least in 1833–4, and in that year, on the contrary, the larger proportion of rain fell in cold months.

Sixthly. *It is probable* that there will be found real differences between the results obtained for daily and nightly periods.

I may now propose a plan of experiments sufficiently within the command of ordinary observers, and likely, if executed contemporaneously at three or more selected points, to furnish a mass of good evidence towards the mathematical investigations which the subject seems to deserve. These should include uniform daily measures of temperature of the air and moist surfaces, amount of evaporation, direction, and mean velocity or amount of wind, and fall of rain at three or four stations, the height of the two or three upper ones being known to feet and inches above the lower one, which should be on the ground. The height of this above the level of the sea should be ascertained. The following is a plan of the register proposed, which includes some occasional observations. The instruments must all be furnished by the Association.

*[Place], N. Lat. [ ], W. Long. [ ], above Sea in Feet [ ], Mean Temp. of the Year [ ].*

| Date. | Barometer and Thermom. | | Wind. | | Ground—above mean level of the sea, feet [ ]. | | | | Middle—above ground, feet [ ]. | | | | Upper—above ground, feet [ ]. | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | Dir. | Vel. | $t$. | $t_{,}$. | Evaporation. | Rain. | $t$. | $t_{,}$. | Evaporation. | Rain. | $t$. | $t_{,}$. | Evaporation. | Rain. |
| 9 A.M. | 29·865 | 60 | E. | 15 | 62 | 58 | | 1·132 | 61 | 57 | | 1·000 | 60 | 56 | | ·800 |
| 9 P.M. | 29·873 | 61 | E. | 10 | 63 | 59 | | 1·731 | 62 | 59 | | 1·500 | 61 | 59 | | 1·200 |

Occasional remarks may be made on approach to perpendicularity of rain; showery or continued rain; size of drops at the different stations; hail, snow, &c. The situations chosen for the experiment should be in open, plain countries, because the influence of the form of undulated ground is extremely uncertain: stations should be selected on opposite sides of the island, as well as in the interior: the central plains of Ireland, and some parts of Scotland, offer excellent points. The elevations of the upper stations above the ground ought to be nearly the same at all the places of experiment, and the highest need not exceed 100 feet, as the following state of results, obtained by the care of my friend Mr. W. D. Littledale, at Bolton Park in Craven, will prove.

| | Wall above ground, 6 ft. 6 in. | House-top, 34 ft. 2 in. | Church Tower, 81 ft. 6 in. |
|---|---|---|---|
| | Inches. | Inches. | Inches. |
| October 1834 to January 1835 | 8·40 | 8·27 | 7·37 |
| March | 2·14 | 1·94 | 1·46 |
| May 15 | 4·77 | 4·70 | 3·92 |
| July 20 | 4·10 | 3·90 | 3·78 |
| | 19·41 | 18·81 | 16·53 |

# *First Report on the Hourly Observations of the Thermometer at the Plymouth Dockyard, Lat.* 50° 21′ *N., Long.* 47° *W. By Mr.* W. S. Harris, *F.R.S., &c.*

At the meeting of the British Association in September, 1831, the Sub-committee of Mathematical and Physical Science deemed it advisable to recommend, "that the Association should employ all the means in its power to procure a Register of the Thermometer during every hour of the day and night, to be kept at some military or naval station in the South of England*," considering that the progress of meteorology materially depends on a thorough acquaintance with the phænomena of diurnal temperature. Soon after this judicious recommendation, two registers were commenced at Devonport near Plymouth; one under the superintendence of the late Mr. Harvey, F.R.S., &c., the other under my own. The former was undertaken at the immediate request of the Association. The latter was offered to its notice more as an individual contribution; it had been already contemplated, and was suggested by a series of meteorological inquiries carried on at Plymouth for several successive years. In speaking of Mr. Harvey, it is impossible to withhold the expression of that just tribute of respect due to the memory of an individual whose talents so greatly contributed to advance him in the estimation of the scientific world. The Association has lost in Mr. Harvey a zealous and able member, whose natural powers were carefully and most industriously cultivated. Had he lived, our Reports would have doubtless been greatly enriched by the results of his labours on this occasion. His register, from the decline of his health, is however, not continued for a sufficient time to render it available to the purpose for which it was undertaken. I have, through the kindness of Mrs. Harvey, been put in possession of all the observations received; these have been carefully discussed and compared †. So far as they go they are of consequence, although only noted for every two hours ‡, since they enable us to observe the influence of local circumstances on the indications of two similar thermometers placed within a short distance of each other.

In order to obtain effectually such an hourly register as that

* *First Report*, p. 49.

† Mr. George Harvey has very kindly assisted in the discussion of these observations.

‡ *Second Report*, p. 571.

in contemplation, an early application was made to Capt. Superintendent Ross, of the Dockyard, who interested himself in the undertaking, and with that kind liberality and courtesy for which he is so greatly distinguished, speedily afforded every just encouragement to the views of the British Association in this great endeavour to advance the interests of science. The hourly register, therefore, was soon commenced and carried on by the Warders and Officers of the Watch stationed at the gate, all persons of respectability and character*. It is due to them to say, that they have entered with more than common interest into the scheme, and have used every exertion to render the observations as perfect as possible. Little difficulty has hence arisen in obtaining the series of thermometric observations in the South of England, as contemplated by the Association, which for extent and accuracy may perhaps be considered the most perfect of any yet recorded.

The thermometer is placed in an insulated position, just within a small circle of grass, about 60 feet above the level of the sea, and distant from it about 300 yards: it is sheltered from direct and indirect radiation, local heat, humidity, and other disturbing causes, by a light screen of latticed wood-work, quite open toward the north-east, and painted of a light colour; this screen is small, and is fixed on a single vertical pillar, as represented in Plate V. The lattices round the sides and beneath are made in the way of Venetian blinds, so that there is always a free current of air in circulation, and little chance of error from the absorption of heat by the screen. The latticed work below inclines at a sufficient angle to cut off all reflexion from the ground. There is a small sliding index of brass attached to the instrument, in the way of a T square; one arm projects over a slate, whilst the other can at any time be made to coincide with the surface of the mercury, and thus show with more precision its exact indications. A line may at the same time be drawn on the slate, coincident with the position of the mercury at the time of an observation; and thus when requisite we may again compare the entries made in the register during the night. The continued habit, however, of registering the observations has rendered this now unnecessary. The instrument itself is a valuable one; it was obtained from Mr. Cox, optician, of Devonport, whose skill in the construction of philo-

* Mr. Isaac Watts, also of the Dockyard, a gentleman of considerable attainments in science, educated at the late school of Naval Architecture at Portsmouth, has been so good as to interest himself in the success of this undertaking, and by an occasional attendance to the progress of the observations has done much in forwarding them.

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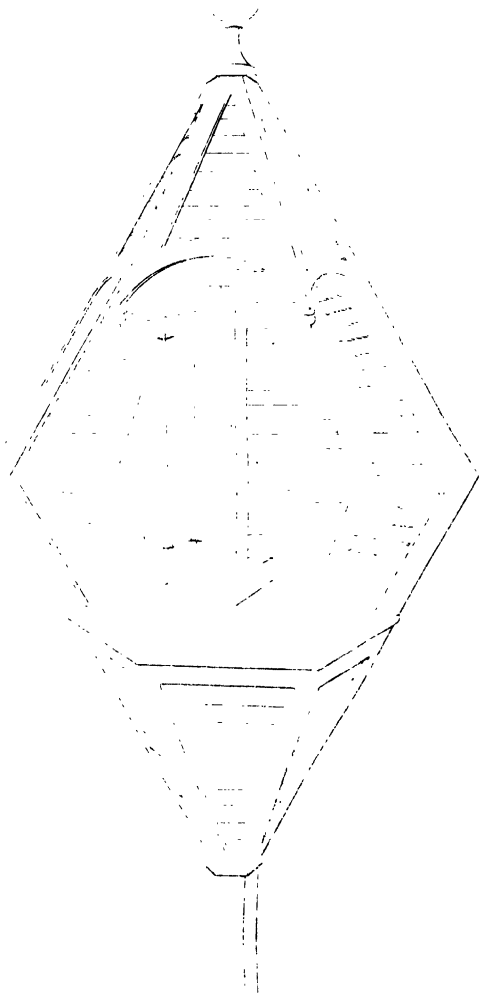


C. Goldsack del

J.W. Lowry Sculp

sophical instruments is well known and appreciated. It agrees well with a fine thermometer by the late Mr. Crichton of Glasgow, made by him expressly for meteorological observations, and with which I am led to believe he took more than ordinary pains.

The thermometer used by Mr. Harvey was also obtained from Mr. Cox, and agreed well with that at the Dockyard. It was placed in the Artillery Square at Devonport in an appropriate screen of wood, fixed against the walls of the line of barracks facing the north, about 90 feet above the level of the sea and distant from it about a quarter of a mile. The sides, back, and roof of the screen were formed of double planks, filled in between with sawdust, so as to shelter the instrument as much as possible from local heat and other disturbing causes*. The non-commissioned officers who went round with the relief guard entered the observations in a printed form most judiciously drawn up and supplied by Mr. Harvey. Acknowledgements are due to Serjeant Anderson of the Artillery, and others under his guidance, for the great care and attention which they bestowed on the register.

On comparing the mean results of the two registers, from their commencement in May, 1832 to the termination of Mr. Harvey's in July, 1833,—or at least such perfect portions as admitted of comparison†,—we find but little difference between them, as may be perceived in the following table which contains the mean temperatures for each month. It will be perceived that the numerical coincidences are very striking, more especially when we take into account the circumstance, that the results in the one case have been deduced from an observation taken every two hours only, the other from hourly observations.

1832.

| Registers. | May. | June. | July. | Aug. | Sept. | Oct. | Nov. | Dec. |
|---|---|---|---|---|---|---|---|---|
| Dockyard ......... | 53·70 | 58·6 | 61·40 | 61·20 | 58·30 | 54·10 | 49·40 | 47·10 |
| Artillery Barracks. | 53·98 | not observed throughout. | 62·20 | 61·95 | 58·27 | 53·20 | 47·97 | not observed throughout. |

1833.

| Registers. | Jan. | Feb. | March. | April. | May. | June. | July. |
|---|---|---|---|---|---|---|---|
| Dockyard ......... | 41·70 | 46·80 | 41·80 | 48·40 | 58· | 57·47 | 61·30 |
| Artillery Barracks. | 40·42 | 45·99 | 41·07 | 48·29 | 59·31 | 57·86 | 61·74 |

* See *Second Report*, p. 574.

† The observations were occasionally interfered with, in consequence of the regiment being sent on military and other service.

I have thought it worth while to allude to this coincidence, as it shows the care with which the respective observations have been made. I find, however, on comparing the registers for particular hours, that some greater differences arise, as might be anticipated from the different local circumstances under which the thermometers were placed; the thermometer in the Dockyard is from its free position less exposed to the influence of local heat than that formerly employed at the Artillery Barracks. It is not, I believe, in any case desirable to place a thermometer so close to the roof of a low building, upon which the sun's rays in summer are powerfully acting, and within which the ordinary avocations of domestic life are carried on; it seems, however, in this case to have been unavoidable. This is, perhaps, alone sufficient to account for the generally small increase of temperature at the Artillery Square shown in the above tables. In the mode of exposure of the thermometer at the Dockyard, I have endeavoured, so far as possible, to fulfill the conditions incidental to the exposure of a thermometer suspended in free space under the shade of a tree, avoiding at the same time the effects of humidity. This is, I believe, by far the most accurate method of observing atmospheric temperature.

The register was commenced on the first day of May, 1832, and has been continued hourly every day and night since, without any intermission. The observations have been fairly copied and reduced up to May, 1835*, so that I am enabled to present the Association with the results of at least two complete years, beginning with January, 1833, and ending with December, 1834. These full years have been selected with a view to an immediate comparison with the results of the hourly register carried on at Leith in the years 1824 and 1825: the observations have been arranged and discussed according to the method resorted to by Sir David Brewster in his capital paper on the Leith Observations in the 10th vol. of the *Transactions of the Royal Society of Edinburgh*.

The months of December, January, and February are taken as winter months; March, April, May, months of spring; June, July, August, summer months; September, October, and November autumnal months. The six months of summer begin with April and end with September, both inclusive; the six

* Mr. T. A. Southwood, of the Classical and Mathematical School at Mount Wise, Devonport, has very kindly afforded me his valuable assistance in discussing the observations.

Mr. Hoskyn also and Mr. G. Harvey have contributed much to advance the work. The cooperation of these gentlemen has been of great consequence, since the labour of copying and reducing above twenty-six thousand observations is by no means small, as is well known to every one engaged in a similar undertaking.

winter months begin with October and end with March, both inclusive. Sir David Brewster seems to have joined October to the winter months, and April and May to the summer months, from having observed a coincidence in the form of the projected curves of temperature of April and May with those of summer, and the curve of October with those of winter. This classification appears natural and sufficiently perfect, and is not opposed to the results of the observations at Plymouth, the daily curve for October being more flattened and of less range than that of April or May, as may be seen by a reference to Plate VII. in which the monthly curves are projected. When the register terminates it may be perhaps desirable to consider the results of other distributions; this may be done with but little additional trouble, since the observations are carefully recorded in large folio volumes, and all the monthly and annual means computed and set down; including intervals of ten days each.

The numerical results for the years 1833 and 1834, of the hourly, daily, and monthly temperatures, as also for periods of three months each, comprising spring, summer, autumn, and winter, likewise summer and winter, including six months each, are given in the eight following tables. From these we may readily deduce:

1. The mean temperature of various seasons, and that of the whole year.
2. The daily progression of temperature.
3. The two periods of each day at which the mean temperature occurs.
4. The relation between the mean temperature of the whole twenty-four hours, and that of any single hour, or of two similar hours.
5. The average daily range for each month.

Lastly, the form of the curves described by the march of the temperature between given periods of the day and night.

## *Hourly Register for* 1833.

### Table I. Containing the Daily and Monthly Mean Temperature for 1833.

| Day. | Jan. | Feb. | Mar. | April. | May. | June. | July. | Aug. | Sept. | Oct. | Nov. | Dec. |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 1 | 43·8 | 44·1 | 43·2 | 50·1 | 49·9 | 61·0 | 54·0 | 63·8 | 52·9 | 53·2 | 54·4 | 52·7 |
| 2 | 46·7 | 48·9 | 50·2 | 49·8 | 53·8 | 57·5 | 55·1 | 60·6 | 54·0 | 53·2 | 51·7 | 49·7 |
| 3 | 40·6 | 50·0 | 50·0 | 51·0 | 53·9 | 52·3 | 56·8 | 61·7 | 56·9 | 53·5 | 49·6 | 50·7 |
| 4 | 37·4 | 52·0 | 47·2 | 50·6 | 57·2 | 54·6 | 58·5 | 58·7 | 54·2 | 56·8 | 45·6 | 53·2 |
| 5 | 36·6 | 51·1 | 47·0 | 46·6 | 57·2 | 57·0 | 60·8 | 58·9 | 54·2 | 52·7 | 50·4 | 47·9 |
| 6 | 40·6 | 50·8 | 43·9 | 48·1 | 60·0 | 57·2 | 62·3 | 59·2 | 57·6 | 53·0 | 54·0 | 46·5 |
| 7 | 41·9 | 50·4 | 43·2 | 47·2 | 59·4 | 59·7 | 58·7 | 59·2 | 58·0 | 54·3 | 48·9 | 48·9 |
| 8 | 41·7 | 50·7 | 39·5 | 46·9 | 59·1 | 60·8 | 60·7 | 61·2 | 59·2 | 53·0 | 46·4 | 48·7 |
| 9 | 40·7 | 46·4 | 36·2 | 51·0 | 54·6 | 60·5 | 61·2 | 60·7 | 61·7 | 55·2 | 19·9 | 54·2 |
| 10 | 45·0 | 50·4 | 38·0 | 49·9 | 54·3 | 60·5 | 66·7 | 60·5 | 59·4 | 55·8 | 53·5 | 47·6 |
| 11 | 45·7 | 47·9 | 38·7 | 47·0 | 57·0 | 57·6 | 61·5 | 60·5 | 58·9 | 55·0 | 53·0 | 43·3 |
| 12 | 45·9 | 50·2 | 36·7 | 46·0 | 58·3 | 56·7 | 60·0 | 59·1 | 55·1 | 51·7 | 49·4 | 43·0 |
| 13 | 45·4 | 48·5 | 37·7 | 47·5 | 56·2 | 56·6 | 61·3 | 57·8 | 57·8 | 53·0 | 50·8 | 46·7 |
| 14 | 45·7 | 47·7 | 40·7 | 47·2 | 59·9 | 57·8 | 59·0 | 57·5 | 59·3 | 57·3 | 41·2 | 48·4 |
| 15 | 41·7 | 10·3 | 38·5 | 45·4 | 63·0 | 57·1 | 60·1 | 57·4 | 57·0 | 49 7 | 43·9 | 52·2 |
| 16 | 39·1 | 41·4 | 44·3 | 43·0 | 57·9 | 59·3 | 63·5 | 58·8 | 57·3 | 50·4 | 51·5 | 52·5 |
| 17 | 41·0 | 49·0 | 43·0 | 41·0 | 57·0 | 58·6 | 64·2 | 58·0 | 56·7 | 50·5 | 54·7 | 48·4 |
| 18 | 40·9 | 43·9 | 42·5 | 44·4 | 56·7 | 58·9 | 64·7 | 60·5 | 56·6 | 52·2 | 50·5 | 50·9 |
| 19 | 38·2 | 43·7 | 41·3 | 16·5 | 57·9 | 57·7 | 62·0 | 62·8 | 58·0 | 49·9 | 46·6 | 53·0 |
| 20 | 39·1 | 44·5 | 42·0 | 51·1 | 57·0 | 55·7 | 59·2 | 63·3 | 54·8 | 51·5 | 51·7 | 49·8 |
| 21 | 38·8 | 45·4 | 40·5 | 51·6 | 55·0 | 58·3 | 59·0 | 63·9 | 57·6 | 56·9 | 52·1 | 43·5 |
| 22 | 38·2 | 44·0 | 39·3 | 50·3 | 61·9 | 58·4 | 60·1 | 60·1 | 58·9 | 56·6 | 53·7 | 50·9 |
| 23 | 41·8 | 43·5 | 37·7 | 51·6 | 64·9 | 56·6 | 59·9 | 59·1 | 58·9 | 53·5 | 48·8 | 53·2 |
| 24 | 39·8 | 47·5 | 38·6 | 51·8 | 65·0 | 55·2 | 60·0 | 59·3 | 59·7 | 58·1 | 47·2 | 51·0 |
| 25 | 41·8 | 46·1 | 40·1 | 49·9 | 65·7 | 54·2 | 59·2 | 59·5 | 57·2 | 58·7 | 42·5 | 47·0 |
| 26 | 41·1 | 45·6 | 40·4 | 49·5 | 58·4 | 53·7 | 63·7 | 58·9 | 54·4 | 56·4 | 43·0 | 48·1 |
| 27 | 39·5 | 43·9 | 40·8 | 51·0 | 55·5 | 56·8 | 65·5 | 57·7 | 53·8 | 57·6 | 49·3 | 47·7 |
| 28 | 47·4 | 44·5 | 42·9 | 49·2 | 56·7 | 56·4 | 68·7 | 60 9 | 55·7 | 58·6 | 48·8 | 47·7 |
| 29 | 44·7 | ... | 43·6 | 46·3 | 57·3 | 59·0 | 66·4 | 60·7 | 54·8 | 59·7 | 48·2 | 53·3 |
| 30 | 41·9 | ... | 42·8 | 49·4 | 58·2 | 58·4 | 68·5 | 58·6 | 53·8 | 58·0 | 49·2 | 52·8 |
| 31 | 40·9 | ... | 47·2 | ... | 60·7 | ... | 63·5 | 56·8 | ... | 55·8 | ... | 50·1 |
| Means | 41·7 | 46·8 | 41·8 | 48·4 | 58·0 | 57·47 | 61·4 | 59·8 | 56·79 | 54·5 | 49·4 | 49·47 |

Mean temp. of the whole year by this table, from 8760 observations, 52·11.

TABLE II. Showing the Mean Temperature of each Hour for each Month of 1833, and for the whole Year.

| Hour. | Jan. | Feb. | Mar. | April. | May. | June. | July. | Aug. | Sept. | Oct. | Nov. | Dec. | Mean annual temp. of each hour. |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 1 A.M. | 40·40 | 46·10 | 38·50 | 44·70 | 52·00 | 53·56 | 56·20 | 54·00 | 52·80 | 51·20 | 47·50 | 48·66 | 48·80 |
| 2 ... | 40·50 | 45·70 | 38·20 | 44·50 | 51·60 | 53·46 | 55·80 | 53·23 | 52·50 | 51·00 | 47·10 | 48·70 | 48·52 |
| 3 ... | 40·20 | 45·00 | 38·10 | 44·30 | 51·50 | 53·50 | 55·40 | 53·10 | 52·40 | 50·80 | 47·10 | 48·70 | 48·31 |
| 4 ... | 40·20 | 45·20 | 37·90 | 43·80 | 51·50 | 53·40 | 55·00 | 52·70 | 52·16 | 50·26 | 46·96 | 48·60 | 48·14 |
| 5 ... | 40·00 | 45·00 | 37·50 | 43·90 | 51·70 | 54·06 | 55·30 | 52·70 | 52·00 | 50·00 | 46·96 | 48·60 | 48·14 |
| 6 ... | 39·70 | 45·10 | 37·60 | 44·20 | 53·40 | 55·86 | 57·40 | 54·00 | 53·26 | 50·30 | 46·70 | 48·76 | 48·85 |
| 7 ... | 39·80 | 45·20 | 38·50 | 46·26 | 56·30 | 57·50 | 59·90 | 56·70 | 54·50 | 51·10 | 47·40 | 49·10 | 50·18 |
| 8 ... | 40·50 | 45·50 | 40·20 | 49·10 | 59·10 | 59·26 | 62·20 | 60·20 | 57·06 | 52·96 | 48·60 | 49·50 | 52·14 |
| 9 ... | 41·20 | 46·60 | 42·80 | 50·90 | 61·00 | 60·30 | 63·70 | 63·20 | 58·90 | 55·96 | 49·86 | 49·70 | 53·67 |
| 10 ... | 42·20 | 47·50 | 45·00 | 51·80 | 62·40 | 60·70 | 65·80 | 65·20 | 61·00 | 58·40 | 51·00 | 50·70 | 55·14 |
| 11 ... | 43·20 | 48·60 | 45·50 | 52·96 | 63·70 | 61·06 | 67·20 | 66·70 | 61·90 | 59·80 | 52·30 | 51·20 | 56·15 |
| 12 ... | 44·30 | 49·80 | 46·70 | 53·70 | 64·30 | 61·40 | 67·60 | 66·60 | 63·06 | 60·56 | 53·60 | 51·70 | 56·95 |
| 1 P.M. | 44·40 | 49·50 | 47·20 | 54·10 | 64·70 | 61·70 | 67·60 | 67·10 | 63·30 | 60·90 | 53·60 | 51·80 | 57·15 |
| 2 ... | 43·90 | 48·90 | 46·70 | 53·70 | 65·00 | 61·70 | 67·00 | 67·10 | 63·30 | 60·10 | 53·70 | 51·30 | 56·86 |
| 3 ... | 43·90 | 48·70 | 46·00 | 53·00 | 64·20 | 60·90 | 67·20 | 66·40 | 62·16 | 59·20 | 52·36 | 50·70 | 56·22 |
| 4 ... | 43·30 | 47·80 | 45·40 | 52·20 | 63·48 | 60·76 | 66·50 | 65·50 | 61·00 | 58·00 | 51·40 | 50·36 | 55·47 |
| 5 ... | 42·60 | 47·20 | 44·50 | 50·86 | 62·10 | 59·70 | 65·30 | 64·40 | 59·36 | 56·46 | 50·20 | 49·26 | 54·32 |
| 6 ... | 42·00 | 46·60 | 43·30 | 49·50 | 60·36 | 58·80 | 63·60 | 62·70 | 57·80 | 54·90 | 49·86 | 49·10 | 53·20 |
| 7 ... | 41·70 | 46·50 | 42·20 | 47·90 | 58·47 | 57·70 | 61·90 | 60·70 | 56·20 | 54·00 | 49·70 | 49·06 | 52·16 |
| 8 ... | 41·20 | 46·50 | 41·50 | 46·80 | 56·87 | 56·90 | 60·30 | 59·30 | 55·46 | 53·50 | 49·30 | 49·06 | 51·38 |
| 9 ... | 41·30 | 46·60 | 40·90 | 46·20 | 55·60 | 55·96 | 58·90 | 57·90 | 54·60 | 52·90 | 48·60 | 49·00 | 50·70 |
| 10 ... | 41·00 | 46·50 | 40·40 | 45·60 | 54·20 | 55·40 | 57·90 | 56·60 | 53·66 | 52·40 | 48·20 | 48·86 | 50·00 |
| 11 ... | 40·80 | 46·40 | 39·70 | 45·40 | 53·89 | 55·10 | 57·30 | 55·70 | 53·16 | 51·86 | 47·80 | 48·86 | 49·65 |
| 12 ... | 40·50 | 46·30 | 39·10 | 44·80 | 53·28 | 54·66 | 56·50 | 54·80 | 52·90 | 51·76 | 47·60 | 48·86 | 49·17 |
| Means | 41·60 | 46·78 | 41·80 | 48·34 | 57·94 | 57·20 | 61·31 | 59·81 | 56·85 | 54·50 | 49·40 | 49·50 | 52·13 |

Mean temp. of the whole year by this table, from 8760 observations, 52·13.

## *Hourly Register for* 1834.

### Table III. Containing the Daily and Monthly Mean Temperatures for 1834.

| Day. | Jan. | Feb. | Mar. | April. | May. | June. | July. | Aug. | Sept. | Oct. | Nov. | Dec. |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 1 | 44·7 | 46·1 | 52·7 | 48·7 | 54·7 | 62·7 | 65·6 | 66·7 | 61·8 | 58·8 | 52·5 | 51·0 |
| 2 | 44·2 | 46·2 | 51·8 | 51·7 | 55·1 | 63·0 | 65·7 | 68·0 | 59·5 | 59·5 | 50·7 | 50·1 |
| 3 | 50·4 | 48·1 | 50·5 | 49·1 | 55·2 | 61·7 | 63·4 | 67·0 | 60·6 | 58·5 | 49·1 | 48·4 |
| 4 | 51·6 | 47·0 | 51·8 | 47·9 | 58·8 | 55·4 | 65·2 | 64·1 | 63·9 | 62·2 | 51·6 | 51·4 |
| 5 | 49·0 | 48·3 | 52·5 | 48·0 | 57·2 | 57·1 | 63·8 | 62·7 | 62·0 | 61·7 | 58·7 | 51·8 |
| 6 | 49·7 | 41·2 | 49·3 | 49·7 | 55·9 | 59·1 | 63·4 | 63·2 | 59·7 | 61·2 | 57·3 | 53·1 |
| 7 | 46·1 | 44·3 | 51·8 | 48·9 | 57·4 | 59·0 | 60·0 | 61·5 | 57·5 | 60·4 | 55·8 | 55·2 |
| 8 | 47·0 | 46·8 | 52·2 | 48·0 | 56·3 | 60·5 | 64·5 | 63·5 | 60·4 | 58·9 | 50·7 | 46·8 |
| 9 | 15·0 | 43·7 | 50·2 | 45·5 | 52·3 | 56·9 | 62·7 | 63·2 | 59·0 | 60·5 | 46·2 | 45·0 |
| 10 | 47·3 | 45·3 | 49·5 | 45·0 | 54·2 | 56·5 | 62·0 | 61·3 | 59·0 | 54·2 | 46·7 | 45·2 |
| 11 | 50·0 | 46·8 | 49·2 | 42·5 | 53·3 | 55·2 | 60·0 | 64·3 | 59·4 | 51·7 | 44·7 | 39·4 |
| 12 | 49·9 | 45·6 | 48·3 | 42·4 | 56·7 | 54·0 | 61·2 | 67·3 | 59·9 | 56·4 | 42·5 | 40·7 |
| 13 | 51·2 | 44·6 | 49·5 | 44·7 | 56·6 | 56·7 | 61·7 | 61·4 | 59·5 | 56·9 | 38·9 | 43·4 |
| 14 | 49·5 | 45·6 | 50·1 | 46·0 | 52·9 | 58·5 | 62·0 | 62·2 | 59·4 | 57·3 | 42·5 | 38·8 |
| 15 | 48·5 | 48·0 | 47·9 | 51·5 | 57·2 | 58·4 | 62·9 | 64·8 | 59·2 | 52·0 | 11·5 | 41·7 |
| 16 | 52·5 | 41·3 | 45·8 | 51·9 | 60·3 | 56·2 | 64·2 | 70·2 | 63·7 | 57·5 | 45·5 | 45·4 |
| 17 | 50·2 | 40·6 | 45·1 | 51·3 | 53·8 | 57·2 | 69·3 | 69·6 | 61·3 | 53·7 | 48·5 | 46·5 |
| 18 | 50·1 | 47·0 | 43·5 | 53·5 | 51·5 | 57·7 | 66·8 | 64·1 | 63·4 | 49·9 | 48·3 | 44·2 |
| 19 | 46·7 | 48·8 | 43·4 | 55·9 | 53·4 | 59·4 | 60·5 | 63·1 | 63·7 | 55·3 | 43·6 | 43·9 |
| 20 | 48·1 | 47·1 | 45·0 | 56·8 | 50·2 | 64·2 | 58·9 | 62·3 | 64·3 | 58·4 | 39·4 | 42·7 |
| 21 | 50·5 | 45·4 | 45·1 | 55·1 | 61·2 | 57·6 | 62·3 | 60 1 | 65·8 | 52·9 | 39·9 | 41·5 |
| 22 | 50·9 | 45·5 | 45·1 | 49·2 | 63·4 | 58·7 | 65·6 | 59·5 | 62·8 | 53·7 | 46·4 | 42·5 |
| 23 | 52·8 | 50·3 | 48·5 | 51·2 | *68·4 | 56·5 | 66·7 | 57·3 | 60·2 | 55·3 | 45·6 | 42·5 |
| 24 | 53·2 | 49·7 | 50·4 | 48·0 | 65·7 | 58·8 | 64·8 | 55·5 | 59·5 | 43·5 | 43·8 | 38·3 |
| 25 | 51·3 | 45·7 | 45·7 | 47·0 | 56·8 | 61·6 | 65·0 | 56·2 | 59·1 | 45·2 | 43·0 | 43·1 |
| 26 | 52·6 | 49·1 | 43·0 | 49·3 | 54·8 | 59·6 | 57·4 | 56·4 | 62·0 | 42·4 | 41·4 | 44·0 |
| 27 | 52·1 | 52·1 | 49·4 | 53·5 | 59·4 | 60·1 | 56·7 | 56·2 | 61·5 | 47·7 | 43·5 | 43·0 |
| 28 | 48·3 | 53·2 | 49·4 | 55·1 | 61·7 | 58·1 | 65·2 | 58·0 | 60·0 | 53·1 | 48·6 | 43·5 |
| 29 | 40·3 | ... | 47·8 | 54·1 | 57·6 | 60·7 | 64·6 | 59·9 | 61·4 | 52·1 | 48·3 | 50·1 |
| 30 | 46·5 | ... | 46·1 | 52·5 | 64·5 | 63·1 | 63·8 | 59·8 | 59·7 | 52·5 | 48·9 | 53·9 |
| 31 | 48·6 | ... | 44·2 | ... | 61·2 | ... | 63·9 | 60·3 | ... | 52·2 | ... | 54·1 |
| Means | 49·0 | 46·80 | 48·2 | 49·8 | 57·3 | 58·8 | 63·2 | 62·3 | 61·0 | 54·7 | 46·9 | 45·8 |

Mean temp. of the whole year by this table, from 8760 observations, 53·65.

TABLE IV. Showing the Mean Temperature of each Hour for each Month of 1834, and for the whole Year.

| Hour. | Jan. | Feb. | Mar. | April. | May. | June. | July. | Aug. | Sept. | Oct. | Nov. | Dec. | Mean annual temp. of each hour. |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 1 A.M. | 48·66 | 45·60 | 46·00 | 44·70 | 51·80 | 54·50 | 58·80 | 58·60 | 57·90 | 51·86 | 44·86 | 44·50 | 50·64 |
| 2 ... | 48·36 | 44·90 | 45·56 | 44·30 | 51·20 | 54·10 | 58·46 | 58·10 | 57·50 | 51·56 | 44·80 | 44·30 | 50·26 |
| 3 ... | 48·20 | 44·40 | 45·40 | 43·80 | 50·80 | 53·66 | 58·26 | 57·70 | 57·20 | 51·30 | 44·90 | 44·20 | 49·98 |
| 4 ... | 48·16 | 44·40 | 45·00 | 43·70 | 50·60 | 53·70 | 58·30 | 57·40 | 57·00 | 51·60 | 44·90 | 44·40 | 49·93 |
| 5 ... | 47·90 | 44·10 | 44·90 | 43·50 | 50·60 | 53·90 | 58·86 | 57·20 | 57·20 | 51·30 | 44·80 | 44·40 | 49·88 |
| 6 ... | 47·50 | 43·90 | 44·96 | 44·30 | 51·70 | 55·80 | 59·90 | 58·50 | 57·40 | 52·30 | 44·76 | 44·40 | 50·45 |
| 7 ... | 47·70 | 43·90 | 45·60 | 46·50 | 55·10 | 58·50 | 62·86 | 60·40 | 58·60 | 53·00 | 45·00 | 44·30 | 51·78 |
| 8 ... | 47·90 | 44·30 | 47·30 | 49·70 | 57·80 | 60·30 | 64·60 | 63·10 | 60·20 | 54·40 | 45·30 | 44·60 | 53·29 |
| 9 ... | 48·76 | 45·80 | 49·10 | 52·30 | 59·70 | 61·76 | 65·96 | 65·40 | 62·70 | 56·50 | 46·80 | 45·20 | 54·99 |
| 10 ... | 49·50 | 47·90 | 50·70 | 54·40 | 61·10 | 62·10 | 66·30 | 66·70 | 64·70 | 58·50 | 48·40 | 46·40 | 56·39 |
| 11 ... | 50·46 | 49·70 | 52·00 | 55·70 | 62·60 | 63·10 | 67·10 | 66·80 | 66·00 | 59·76 | 49·60 | 47·60 | 57·53 |
| 12 ... | 50·80 | 50·50 | 52·90 | 56·90 | 63·70 | 63·40 | 67·90 | 67·50 | 67·10 | 60·10 | 50·60 | 49·30 | 58·39 |
| 1 P.M. | 50·90 | 51·20 | 53·40 | 57·70 | 64·40 | 63·80 | 68·46 | 67·90 | 67·50 | 60·10 | 51·30 | 49·60 | 58·85 |
| 2 ... | 50·46 | 50·90 | 52·66 | 57·40 | 64·10 | 63·56 | 68·10 | 67·90 | 66·90 | 59·90 | 50·70 | 48·80 | 58·44 |
| 3 ... | 50·00 | 50·30 | 52·10 | 56·60 | 63·30 | 63·40 | 68·00 | 67·70 | 66·00 | 58·70 | 50·00 | 48·30 | 57·86 |
| 4 ... | 49·80 | 49·30 | 51·20 | 54·70 | 62·40 | 62·46 | 67·36 | 66·70 | 64·40 | 57·30 | 48·60 | 47·20 | 56·78 |
| 5 ... | 49·36 | 47·60 | 50·36 | 53·40 | 61·60 | 62·10 | 66·16 | 65·60 | 62·90 | 56·00 | 47·40 | 46·20 | 55·72 |
| 6 ... | 49·10 | 47·50 | 49·10 | 51·50 | 59·60 | 60·90 | 65·26 | 64·40 | 61·50 | 54·96 | 46·80 | 45·70 | 54·69 |
| 7 ... | 49·20 | 46·80 | 47·76 | 49·40 | 57·70 | 59·56 | 63·86 | 62·80 | 60·30 | 53·90 | 46·80 | 45·40 | 53·64 |
| 8 ... | 48·60 | 46·40 | 47·30 | 48·70 | 56·50 | 57·70 | 62·10 | 61·10 | 59·10 | 53·56 | 46·40 | 45·20 | 52·72 |
| 9 ... | 48·50 | 46·10 | 46·76 | 48·00 | 55·30 | 56·80 | 60·70 | 60·20 | 59·00 | 53·20 | 46·20 | 45·10 | 52·21 |
| 10 ... | 48·70 | 45·90 | 46·50 | 46·40 | 54·00 | 56·10 | 60·10 | 59·60 | 58·50 | 52·80 | 45·90 | 45·20 | 51·64 |
| 11 ... | 48·76 | 46·10 | 46·20 | 45·70 | 53·20 | 55·50 | 59·46 | 59·40 | 58·30 | 52·30 | 45·46 | 44·90 | 51·27 |
| 12 ... | 48·70 | 46·00 | 46·00 | 45·20 | 52·60 | 54·80 | 58·90 | 58·80 | 58·20 | 52·20 | 45·40 | 44·70 | 50·95 |
| Means | 48·99 | 46·80 | 48·28 | 49·70 | 57·10 | 58·80 | 63·15 | 62·50 | 61·08 | 54·87 | 46·98 | 45·80 | 53·67 |

Mean temp. of the whole year by this table, from 8760 observations 53·67.

*Mean Hourly Registers for the Years* 1833 *and* 1834.

TABLE V. Containing the Daily and Monthly Mean Temperatures for 1833 and 1834.

| Day. | Jan. | Feb. | Mar. | April. | May. | June. | July. | Aug. | Sept. | Oct. | Nov. | Dec. |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 1 | 44·25 | 45·10 | 47·95 | 49·40 | 52·30 | 61·85 | 59·80 | 65·25 | 57·35 | 56·00 | 53·45 | 51·85 |
| 2 | 45·45 | 47·55 | 51·00 | 45·75 | 54·45 | 60·25 | 60·40 | 64·30 | 56·75 | 56·35 | 51·20 | 49·90 |
| 3 | 45·50 | 49·05 | 50·25 | 50·05 | 54·55 | 57·00 | 60·10 | 64·35 | 58·75 | 56·00 | 49·35 | 49·55 |
| 4 | 44·30 | 49·50 | 49·50 | 49·25 | 58·00 | 55·00 | 61·85 | 61·40 | 59·05 | 59·50 | 48·60 | 52·30 |
| 5 | 42·80 | 49·70 | 49·75 | 47·30 | 57·20 | 57·05 | 62·30 | 60·80 | 58·10 | 57·20 | 54·55 | 49·85 |
| 6 | 45·15 | 47·50 | 46·60 | 48·90 | 57·95 | 58·15 | 62·85 | 61·20 | 58·65 | 57·10 | 55·65 | 49·80 |
| 7 | 44·15 | 47·35 | 47·00 | 48·05 | 58·40 | 59·35 | 59·35 | 60·35 | 57·75 | 57·35 | 52·35 | 52·05 |
| 8 | 44·35 | 48·75 | 45·85 | 47·45 | 57·70 | 60·65 | 62·60 | 62·35 | 59·80 | 55·95 | 48·55 | 47·75 |
| 9 | 42·85 | 45·05 | 43·20 | 48·25 | 53·45 | 58·70 | 61·95 | 61·95 | 60·35 | 57·85 | 48·05 | 49·60 |
| 10 | 46·15 | 47·85 | 43·75 | 47·45 | 54·25 | 58·50 | 64·35 | 60·90 | 59·20 | 55·00 | 50·10 | 46·40 |
| 11 | 47·85 | 47·30 | 43·95 | 44·75 | 55·15 | 56·40 | 60·75 | 62·40 | 59·16 | 53·35 | 48·85 | 46·35 |
| 12 | 47·90 | 47·85 | 42·50 | 44·20 | 57·50 | 55·35 | 60·60 | 63·20 | 57·50 | 54·05 | 45·95 | 41·85 |
| 13 | 48·30 | 46·30 | 43·60 | 46·10 | 56·40 | 56·65 | 61·50 | 61·10 | 58·40 | 54·95 | 44·85 | 45·05 |
| 14 | 47·60 | 46·65 | 45·40 | 46·60 | 56·40 | 58·15 | 60·50 | 59·85 | 59·35 | 57·30 | 43·35 | 43·60 |
| 15 | 45·10 | 44·15 | 43·20 | 48·45 | 60·10 | 57·75 | 61·50 | 61·10 | 58·10 | 50·85 | 44·20 | 46·95 |
| 16 | 45·80 | 41·35 | 45·05 | 47·45 | 59·10 | 57·75 | 63·85 | 64·50 | 60·50 | 53·95 | 48·50 | 48·95 |
| 17 | 45·60 | 44·80 | 44·05 | 47·65 | 55·40 | 57·90 | 66·75 | 63·80 | 60·50 | 52·10 | 51·60 | 47·45 |
| 18 | 45·50 | 45·45 | 43·00 | 48·95 | 54·10 | 58·30 | 65·75 | 62·30 | 60·00 | 51·05 | 49·40 | 47·55 |
| 19 | 42·45 | 46·30 | 42·35 | 51·20 | 55·65 | 58·55 | 61·25 | 62·95 | 60·85 | 52·60 | 45·10 | 48·45 |
| 20 | 43·60 | 45·80 | 43·50 | 53·95 | 53·60 | 59·95 | 59·05 | 62·80 | 59·55 | 54·95 | 45·55 | 46·25 |
| 21 | 44·65 | 43·30 | 42·80 | 53·35 | 58·10 | 57·95 | 61·45 | 62·00 | 61·70 | 54·90 | 46·00 | 42·50 |
| 22 | 44·55 | 44·75 | 42·20 | 49·75 | 62·65 | 58·55 | 62·85 | 59·80 | 60·85 | 55·15 | 50·05 | 46·70 |
| 23 | 47·30 | 46·90 | 43·10 | 51·40 | 66·65 | 56·55 | 63·30 | 58·20 | 59·55 | 54·40 | 47·20 | 47·85 |
| 24 | 46·50 | 48·60 | 44·50 | 49·90 | 65·30 | 57·00 | 62·40 | 57·40 | 59·60 | 50·80 | 45·50 | 44·65 |
| 25 | 46·55 | 45·90 | 42·90 | 48·45 | 61·25 | 57·90 | 62·10 | 57·85 | 58·30 | 51·95 | 42·75 | 45·05 |
| 26 | 46·85 | 47·35 | 41·70 | 49·40 | 56·60 | 56·65 | 60·55 | 57·65 | 58·20 | 49·40 | 42·20 | 46·05 |
| 27 | 45·80 | 48·00 | 45·10 | 52·25 | 57·45 | 58·45 | 61·10 | 56·95 | 57·65 | 52·65 | 46·40 | 45·35 |
| 28 | 47·85 | 48·85 | 46·15 | 52·15 | 59·20 | 57·25 | 65·15 | 59·45 | 57·85 | 55·85 | 48·70 | 45·60 |
| 29 | 42·50 | ... | 45·70 | 50·20 | 57·45 | 59·85 | 65·50 | 60·30 | 58·10 | 55·90 | 48·25 | 51·70 |
| 30 | 44·20 | ... | 44·45 | 50·95 | 61·35 | 60·75 | 66·15 | 59·20 | 56·75 | 55·25 | 49·05 | 53·35 |
| 31 | 44·75 | ... | 45·70 | ... | 60·95 | ... | 63·70 | 58·55 | ... | 54·00 | ... | 52·10 |
| Means | 45·36 | 46·67 | 45·02 | 48·96 | 57·70 | 58·15 | 62·30 | 61·10 | 58·94 | 54·63 | 48·17 | 47·64 |

Mean temp. of 1833 and 1834 by this table, from 17520 observations, 52·85.

TABLE VI. Showing the Mean Temperature of each Hour for each Month of 1833 and 1834, and for the Two Years.

| Hour. | Jan. | Feb. | Mar. | April. | May. | June. | July. | Aug. | Sept. | Oct. | Nov. | Dec. | Mean annual temp. of each hour. |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 1 A.M. | 44·53 | 45·85 | 42·25 | 44·70 | 51·90 | 54·03 | 57·50 | 56·30 | 55·35 | 51·53 | 46·18 | 46·58 | 49·72 |
| 2 ... | 44·43 | 45·30 | 41·88 | 44·40 | 51·40 | 53·78 | 57·13 | 55·66 | 55·00 | 51·28 | 45·95 | 46·50 | 49·39 |
| 3 ... | 44·20 | 44·70 | 41·75 | 44·05 | 51·15 | 53·58 | 56·83 | 55·40 | 54·85 | 51·05 | 46·00 | 46·45 | 49·16 |
| 4 ... | 44·18 | 44·80 | 41·45 | 43·75 | 51·05 | 53·55 | 56·65 | 55·05 | 54·58 | 50·93 | 45·93 | 46·50 | 49·03 |
| 5 ... | 43·95 | 44·50 | 41·20 | 43·70 | 51·15 | 53·98 | 57·08 | 54·95 | 54·60 | 50·65 | 45·88 | 46·50 | 49·00 |
| 6 ... | 43·60 | 44·50 | 41·28 | 44·25 | 52·55 | 55·83 | 58·65 | 56·25 | 55·33 | 51·30 | 45·73 | 46·58 | 49·65 |
| 7 ... | 43·75 | 44·55 | 42·05 | 46·38 | 55·70 | 58·00 | 61·38 | 58·55 | 56·55 | 52·05 | 46·20 | 46·70 | 50·98 |
| 8 ... | 44·20 | 44·90 | 43·75 | 49·40 | 58·45 | 59·78 | 63·40 | 61·60 | 58·63 | 53·68 | 46·90 | 47·05 | 52·63 |
| 9 ... | 44·98 | 46·20 | 45·90 | 51·60 | 60·35 | 61·03 | 64·83 | 64·30 | 60·80 | 56·23 | 48·33 | 47·45 | 54·33 |
| 10 ... | 45·85 | 47·70 | 47·85 | 53·10 | 61·75 | 61·40 | 66·05 | 65·95 | 62·85 | 58·45 | 49·70 | 48·55 | 55·76 |
| 11 ... | 46·83 | 49·15 | 48·75 | 54·33 | 63·00 | 62·08 | 67·15 | 66·75 | 63·95 | 59·78 | 50·95 | 49·40 | 56·84 |
| 12 ... | 47·55 | 50·15 | 49·80 | 55·30 | 64·05 | 62·40 | 67·75 | 67·05 | 65·08 | 60·33 | 52·10 | 50·50 | 57·67 |
| 1 P.M. | 47·65 | 50·35 | 50·30 | 55·90 | 64·55 | 62·75 | 68·03 | 67·50 | 65·40 | 60·50 | 52·45 | 50·70 | 58·00 |
| 2 ... | 47·18 | 49·90 | 49·68 | 55·55 | 64·55 | 62·63 | 67·90 | 67·50 | 65·10 | 60·00 | 52·20 | 50·05 | 57·68 |
| 3 ... | 46·95 | 49·50 | 49·05 | 54·80 | 63·75 | 62·15 | 67·60 | 67·05 | 64·08 | 58·95 | 51·18 | 49·50 | 57·04 |
| 4 ... | 46·55 | 48·55 | 48·30 | 53·45 | 62·94 | 61·61 | 66·93 | 66·10 | 62·70 | 57·65 | 50·00 | 48·78 | 56·12 |
| 5 ... | 45·98 | 47·40 | 47·43 | 52·13 | 61·85 | 60·90 | 65·73 | 65·00 | 61·13 | 56·23 | 48·80 | 47·73 | 55·02 |
| 6 ... | 45·55 | 47·05 | 46·20 | 50·50 | 59·95 | 59·85 | 64·43 | 63·50 | 59·65 | 54·93 | 48·33 | 47·40 | 53·94 |
| 7 ... | 45·45 | 46·65 | 44·98 | 48·65 | 58·08 | 58·63 | 62·88 | 61·75 | 58·25 | 53·95 | 48·20 | 47·23 | 52·90 |
| 8 ... | 44·90 | 46·45 | 44·40 | 47·75 | 56·65 | 57·30 | 61·20 | 60·20 | 57·28 | 53·53 | 47·85 | 47·13 | 52·05 |
| 9 ... | 44·90 | 46·35 | 43·83 | 47·10 | 55·45 | 56·38 | 59·80 | 59·05 | 56·80 | 53·05 | 47·40 | 47·05 | 51·45 |
| 10 ... | 44·85 | 46·20 | 43·45 | 46·00 | 54·10 | 55·75 | 59·00 | 58·10 | 56·08 | 52·60 | 47·05 | 47·03 | 50·85 |
| 11 ... | 44·78 | 46·25 | 42·95 | 45·55 | 53·55 | 55·30 | 58·38 | 57·55 | 55·73 | 52·08 | 46·63 | 46·88 | 50·54 |
| 12 ... | 44·60 | 46·15 | 42·55 | 45·00 | 52·95 | 54·73 | 57·70 | 56·80 | 55·55 | 51·98 | 46·50 | 46·75 | 50·11 |
| Means | 45·30 | 46·79 | 45·04 | 49·06 | 57·53 | 58·16 | 62·20 | 61·16 | 58·97 | 54·69 | 48·19 | 47·72 | 52·90 |

Mean temp. of 1833 and 1834 by this table, from 17520 observations, 52·90.

Table VII. Showing the Mean Hourly Temperature for each of the Seasons, viz. Spring, Summer, Autumn, and Winter, for the Years 1833 and 1834.

| Hour. | Spring. | Summer. | Autumn. | Winter. |
|---|---|---|---|---|
| 1 A.M. | 46·28 | 55·94 | 51·02 | 45·65 |
| 2 ... | 45·89 | 55·92 | 50·74 | 45·37 |
| 3 ... | 45·65 | 55·27 | 50·63 | 45·11 |
| 4 ... | 45·41 | 55·07 | 50·48 | 45·16 |
| 5 ... | 45·35 | 55·33 | 50·35 | 44·98 |
| 6 ... | 46·02 | 56·89 | 50·78 | 44·89 |
| 7 ... | 48·04 | 59·31 | 51·60 | 45·00 |
| 8 ... | 50·53 | 61·59 | 53·07 | 45·43 |
| 9 ... | 52·61 | 63·38 | 55·12 | 46·21 |
| 10 ... | 54·23 | 64·46 | 57·00 | 47·36 |
| 11 ... | 55·36 | 65·32 | 58·22 | 48·46 |
| 12 ... | 56·38 | 65·73 | 59·17 | 49·10 |
| 1 P.M. | 56·91 | 66·09 | 59·51 | 49·56 |
| 2 ... | 56·59 | 66·01 | 59·10 | 49·01 |
| 3 ... | 55·86 | 65·60 | 58·07 | 48·65 |
| 4 ... | 54·89 | 64·88 | 56·78 | 47·96 |
| 5 ... | 53·80 | 63·87 | 55·38 | 47·03 |
| 6 ... | 52·21 | 62·59 | 54·30 | 46·66 |
| 7 ... | 50·57 | 61·08 | 53·46 | 46·44 |
| 8 ... | 49·60 | 59·56 | 52·88 | 46·16 |
| 9 ... | 48·79 | 58·41 | 52·41 | 46·10 |
| 10 ... | 47·85 | 57·61 | 51·91 | 46·02 |
| 11 ... | 47·35 | 57·07 | 51·48 | 45·93 |
| 12 ... | 46·83 | 56·41 | 51·34 | 45·83 |
| Means | 50·55 | 60·55 | 53·95 | 46·55 |

Mean temp. of the two years by this table, from 17520 observations, 52·90.

TABLE VIII. Showing the Mean Hourly Temperature for the Six Summer Months, from April to September inclusive, and for the Six Winter Months, from October to March inclusive, in the Years 1833 and 1834.

| Hours. | Summer Months. | Winter Months. |
|---|---|---|
| 1 A.M. | 53·29 | 46·15 |
| 2 ... | 52·89 | 45·88 |
| 3 ... | 52·63 | 45·69 |
| 4 ... | 52·43 | 45·63 |
| 5 ... | 52·57 | 45·45 |
| 6 ... | 53·52 | 45·49 |
| 7 ... | 56·08 | 45·88 |
| 8 ... | 58·27 | 46·78 |
| 9 ... | 60·48 | 48·18 |
| 10 ... | 61·85 | 49·68 |
| 11 ... | 62·87 | 50·81 |
| 12 ... | 63·60 | 51·73 |
| 1 P.M. | 64·02 | 51·99 |
| 2 ... | 63·81 | 51·49 |
| 3 ... | 63·23 | 50·84 |
| 4 ... | 62·28 | 49·96 |
| 5 ... | 61·12 | 48·92 |
| 6 ... | 59·65 | 48·24 |
| 7 ... | 58·03 | 47·77 |
| 8 ... | 56·72 | 47·37 |
| 9 ... | 55·76 | 47·13 |
| 10 ... | 54·85 | 46·86 |
| 11 ... | 54·08 | 46·59 |
| 12 ... | 53·80 | 46·42 |
| Means | 57·826 | 47·955 |

Mean temp. of the two years by this table, from 17520 observations, 52·89.

The hourly observations for 1833 and 1834 having been thus discussed, and the mean results disposed as in the eight preceding tables, we may now proceed to consider the general results deducible from them.

### *Periods of Maximum and Minimum of Temperature; Mean Temperature of the four Seasons of Summer and Winter, and of the whole Year.*

#### 1833.

Maximum 80, took place July 30, at 2 P.M.
Minimum 28, occurred March 13, at 5, 6, 7 A.M.

| | | |
|---|---|---|
| Mean temperature of 10 days about the summer solstice, viz. from June 15 to June 25 | | 57·58 |
| Mean temperature of 10 days about the winter solstice, viz. from December 15 to December 25 | | 50·02 |
| Mean temperature of the four seasons, from 2190 observations by Table III., including 3 months each | Winter, viz. Jan., Feb., Dec. | 46· |
| | Spring, viz. March, April, May | 49·40 |
| | Summer, viz. June, July, August | 59·52 |
| | Autumn, viz. Sept., Oct., Nov. | 53·56 |
| Mean temperature of winter and summer, from 4380 observations by Table IV., including 6 months each | Winter, viz. Jan., Feb., March, Oct., Nov., Dec. | 47·27 |
| | Summer, viz. April, May, June, July, Aug., Sept. | 56·90 |
| Mean temperature of the whole year, from 8760 observations by Tables I. and II. | | 52·12 |
| Mean temperature of October, being the nearest to that of the year | | 51·50 |
| Extreme range of temperature | | 52· |
| Mean range of temperature | | 9·63 |

#### 1834.

Maximum 80, occurred July 17, at 4 P.M.
Minimum 32, occurred { February 17, at 3, 4, 5, 6, 7 A.M. / March 26, at 2, 3, 4, 5, 6 A.M.

| | | |
|---|---|---|
| Mean temperature of 10 days about the summer solstice, viz. from June 15 to June 25 | | 58·79 |
| Mean temperature of 10 days about the winter solstice, viz. from December 15 to December 25 | | 43·06 |
| Mean temperature of the four seasons, from 2190 observations, by Table III., including 3 months each | Winter, viz. Jan., Feb., Dec. | 47· |
| | Spring, viz. March, April, May | 51·76 |
| | Summer, viz. June, July, August | 61·43 |
| | Autumn, viz. Sept., Oct., Nov. | 54·20 |
| Mean temperature of winter and summer, from 4380 observations, by Table IV., including 6 months each | Winter, viz. Jan., Feb., March, Oct., Nov., Dec. | 48·56 |
| | Summer, viz. April, May, June, July, Aug., Sept. | 58·73 |
| Mean temperature of the whole year, from 8760 observations, by Tables III. and IV. | | 53·66 |
| Mean temperature of October, being the nearest to that of the year | | 54·700 |
| Extreme range of temperature | | 48· |
| Mean range of temperature | | 10·18 |

## 1833 and 1834.

| | | |
|---|---|---|
| Mean maximum | | 80· |
| Mean minimum | | 30· |
| Mean temperature of 10 days about the summer solstice | | 58·185 |
| Mean temperature of 10 days about the winter solstice | | 46·540 |
| Mean temperature of the four seasons, from 4380 observations, by Table VII., including 3 months each | Winter, viz. Jan., Feb., Dec. | 46·57 |
| | Spring, viz. March, April, May | 50·55 |
| | Summer, viz. June, July, August | 60·50 |
| | Autumn, viz. Sept., Oct., Nov. | 53·93 |
| Mean temperature of winter and summer, from 8760 observations, by Table VIII., including 6 months each | Winter, viz. Jan., Feb., March, Oct., Nov., Dec. | 47·955 |
| | Summer, viz. April, May, June, July, Aug., Sept., | 57·826 |
| Mean temperature of the two years, from 17,520 observations, by Table VII. | | 52·90 |
| Mean temperature of October, the nearest to the mean of the two years | | 54·60 |
| Extreme range from the mean of the two years | | 50· |
| Mean range from the mean of the two years | | 9·871 |

### *Daily Progression of Temperature.*

The daily temperature for each hour for each of the years 1833 and 1834, together with the mean of the two years, is projected under the form of curves in Plate VI., from the numbers in the last columns of Tables II., IV., and VI. The dotted curved lines represent the progress of the daily temperature at Leith.

Each point of the mean curve is the result of 730 observations, and each point of the curves for the separate years the result of 365 observations. From these it appears that the temperature is lowest at 5 A.M., after which it steadily increases until 1 P.M., when it again descends to the minimum. The period of ascent being eight hours, and that of the descent sixteen hours, which numbers are in the ratio of 1 : 2. Taking the rapidity with which the temperature increases or decreases between the points of maximum and minimum, through a given number of degrees, as inversely proportional to the times, we may conclude, in neglecting the intermediate changes, that *upon the whole* the heat of the day advances in this case with twice the rapidity of the cold of the night. The general agreement between the curves of the two years and the mean is very striking, and shows a close approximation in the observations to the true form of the daily curve. On comparing these results with the hourly register at Leith Fort for the years 1824 and 1825, we observe, as shown by the dotted curved lines in Plate I., that the maximum does not take place at Leith until two hours after it occurs at Plymouth, whilst the minimum occurs at nearly the same hour. The curves also are, as might be antici-

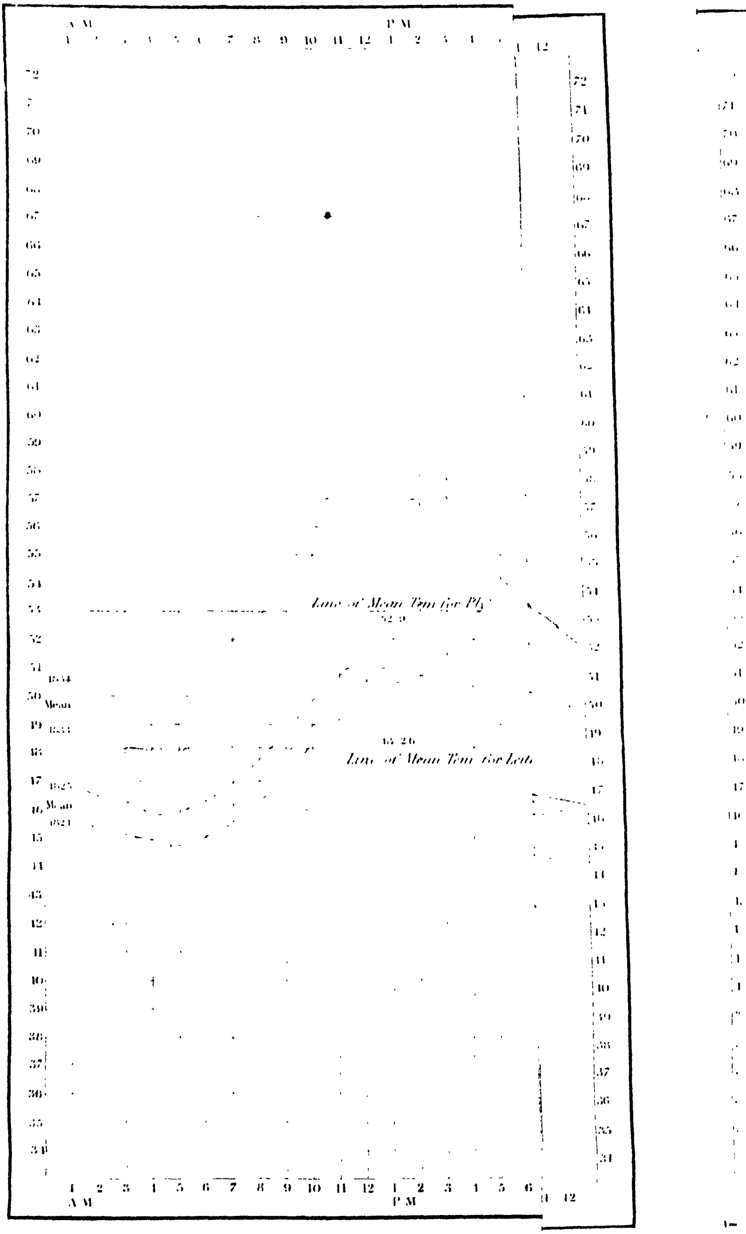
pated, more flattened as we advance northward, and lower in the scale.

The ascending motion in the Leith curves is 9 hours 40 minutes, and the descending motion 14 hours 20 minutes, which are to each other in the ratio of 2 : 3 nearly. The heat of the day in this case advances one third more rapidly than the cold of the night, but not so rapidly as at Plymouth, in the proportion of 1 : 3.

The form of the daily curves for the different months and the four seasons is seen in Plates VII. and VIII. They are projected from the mean results in Tables VI. and VII.

In Plate VII. the curves evidently show three distinct varieties of temperature, as indicated by a general coincidence in the blue, red, yellow, and black lines, constituting the curves of spring, summer, autumn, and winter. These are more completely reduced in Plate VIII., in which we immediately distinguish the sharp, full curve of spring; the collapsing and less sharpened curve of autumn; the full, broad, rounded curve of summer; and the low, flattened curve of winter. The autumnal curve evidently approaches more nearly to the curve of winter: its sides, instead of presenting a full appearance, rather tend to fall in, and assimilate with, the form of the winter curve; this is especially evident in the afternoon branch. The curve of spring, on the contrary, has its branches fuller and more rounded, and evidently approaches in general character the curve of summer. These peculiarities in the general form of the respective curves are also observable in Plate VII. and are well marked in the curves of April and October; hence it seems more natural to class April with the summer months, and October with the winter. The respective differences and agreements in the curves of these months, which have been considered by many meteorologists as having a mean temperature equal to that of the whole year, are not unworthy of notice: they both approach the form and character of the annual curve, and to a certain extent resemble each other; they differ, however, greatly in other respects. The general mean of October is above that of April, but the range of temperature is less. The mornings and evenings of April are much colder than those of October, which are comparatively warm, and far above the mean temperature of the year. April, as observed at Leith, unites the low temperature of winter with the high range of summer; whilst October unites the high temperature of summer with the low range of winter. The great regularity of the curves of these months, as shown in Plate VII. as also the regularity of the mean curves observable in Plate IX. are very striking and remarkable.

PLATE 6



In Plate IX. are projected the mean summer and winter curves, from the two columns of Table VIII. These are extremely regular, more especially the former: each point of each curve is the mean of 365 observations. The mean annual curve is also shown between these as projected from the last column of Table VI.

The summer curve descends regularly from midnight until 4 A.M., when the greatest cold occurs; it then ascends with extraordinary rapidity and regularity until 1 P.M., after which it descends with great regularity, but with less rapidity, to its minimum; the total mean range being about 11 degrees and a half. The period of the ascent is nine hours, and the descent fifteen hours, which numbers are to each other as 3 : 5. The heat of the day in summer, therefore, increases at Plymouth faster than the cold of the night in the ratio of 5 : 3. It is interesting to observe the difference between this and the summer curve at Leith, which rises less rapidly, and somewhat less regularly, until 3 P.M., after which it finally descends with rapidity to the minimum; the total mean range being about 8·5°. The period of its ascent is eleven hours, and the descent thirteen hours; hence the heat of the day increases in summer at Leith more rapidly upon the whole than the cold of the night in the ratio of 13 : 11, being less than the rate of increase at Plymouth by nearly one fourth.

The winter curve for Plymouth descends regularly from midnight until 6 A.M., at which time the cold is greatest: from this time it rises until 1 P.M., as before, the period of its ascent being seven hours, and that of its descent seventeen hours: hence we may conclude, in neglecting the intermediate gradations as before, that upon the whole the heat of the day advances in winter at Plymouth more rapidly than the cold of the night in the ratio of 17 : 7, or as 5 : 3 nearly, being about $\frac{5}{6}$ times more rapidly than in summer. In the winter curve for Leith there is a gentle rise of temperature after midnight, which is not apparent at Plymouth. On comparing those curves in Plate IX. with the analogous curves of Sir David Brewster for Leith as shown by the dotted lines, we find some interesting points of difference. The afternoon branches of the Leith curves, for example, especially those of summer, as also the mean curve, are more full and protrusive than the similar curves for Plymouth. In the individual curves for Leith which compose the winter group, and from which the dotted yellow and blue lines in Plate IX. are deduced, there is a rise of temperature after midnight, and then a subsequent fall; this, however, does not seem ever to occur at Plymouth, nor does it occur in the summer curves for Leith.

### *On the Periods of Time at which the Annual Daily and other Curves cross the line of Mean Temperature.*

If we consider the march of temperature through the day, it is obvious that the temperature of certain periods is the same as that of the mean temperature of the whole twenty-four hours; and since the thermometer increases and decreases regularly once in each day, we may infer that the periods of mean temperature are generally two, one of them being in some point of the increase at morning, the other at some point of the decrease at evening. We may extend this consideration in a similar way to other periods greater than a day; such as a month, three months, six months, or a whole year. And hence, if a line of mean temperature be drawn for the respective curves in Plates VI., VIII., IX., &c., it is evident that each of these curves will cross that line twice in the course of their progress. We shall first, by references to the preceding tables and plates, endeavour to determine the particular hours, the mean temperature of which, as deduced by observation through the whole year, is equal to the mean temperature of the whole twenty-four hours, as deduced from similar observations.

The determination of the two times of the day at which the mean temperature happens, is an object of great interest in the science of meteorology; it enables an observer to arrive at the mean temperature of any given place by two observations, or even one, each day. These critical times, however*, may vary materially, both with the latitude and height above the sea: hence the necessity of ascertaining by extensive observation the laws of this variation.

It appears from Table VII. that the mean temperature of Plymouth, for the whole year, is 52·90; we may take this as the average mean temperature of twenty-four hours, as shown in the last column of Table VI. Now the critical times in the twenty-four hours at which this mean temperature happens, will, from Tables II., IV., and VI., be as follow:

TABLE IX.

| Years. | Times of Morning Mean Temperature. | Times of Evening Mean Temperature. |
|---|---|---|
| | h m | h m |
| 1833. | 7 59 | 7 3 |
| 1834. | 8 13 | 6 58 |
| Mean ................. | 8 6 | 7 |
| Mean by Table VI. | 8 9 | 7 |

* *Edinburgh Phil. Trans.* vol. x. p. 379.

We observe here a considerable approximation to the mean in the results of each year, and may therefore conclude that the mean daily curve, as exhibited by the yellow line in Plate VI. will cross the line of mean temperature about $8^h\ 9^m$ A.M. and $7^h$ P.M., the interval being $10^h\ 51^m$.

Although these critical times may be greatly influenced by latitude, &c., yet the differences in the interval which elapses between the morning and evening mean at different places seems comparatively inconsiderable. I have arranged in the following table the times, &c., of mean temperature for a few places at which these elements have been determined, and by which it will be perceived that the interval amounts nearly to eleven hours.

TABLE X.

| Place. | Latitude. | Longitude. | Height above the Sea. | Distance from Sea. | Time of Morning Mean. | Time of Evening Mean. | Interval between Morning and Evening Mean. |
|---|---|---|---|---|---|---|---|
| | | | feet. | feet. | h m | h m | h m |
| Leith ......... | 55·56 | 3·13 W. | 25 | 600 | 9 13 | 8 27 | 11 14 |
| Plymouth ... | 50·21 | 4·6 | 60 | 100 | 8 9 | 7 | 10 51 |
| Padua ........ | 45·36 | 11·55 E. | ... | ... | 8 41 | 7 52 | 11 11 |

It is to be observed, that of these places the comparison between Plymouth and Leith is the most accurate, since the observations are in each case compared with the mean temperature of the twenty-four hours, and not with a mean temperature obtained in a more general way.

The hours of mean temperature which we have been just considering being deduced from the mean results of the whole year, do not consequently apply to a less period of time. It may not, therefore, be unnecessary to determine the critical hours for other periods of time less than a year. The principal of these are given in the three following tables.

TABLE XI.—Showing the Hours of Morning and Evening when the Mean Monthly Temperature occurs, as deduced by Table VI.

| Mean of 1833 & 1834. | Jan. | Feb. | Mar. | April. | May. | June. | July. | Aug. | Sept. | Oct. | Nov. | Dec. |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | h m | h m | h m | h m | h m | h m | h m | h m | h m | h m | h m | h m |
| A.M. | 9 22 | 9 23 | 8 36 | 7 52 | 7 39 | 7 50 | 7 21 | 7 51 | 8 9 | 8 23 | 8 54 | 9 14 |
| P.M. | 7 16 | 6 39 | 6 58 | 6 47 | 7 25 | 7 21 | 7 25 | 7 23 | 6 28 | 6 15 | 7 20 | 5 20 |

TABLE XII.—Showing the Hours of Morning and Evening when the Mean Temperature of Spring, Summer, Autumn, and Winter occurs in each season, as deduced by Table VII.

| Mean of 1833 and 1834. | Spring. | | Summer. | | Autumn. | | Winter. | |
|---|---|---|---|---|---|---|---|---|
| | h | m | h | m | h | m | h | m |
| A.M. | 8 | 1 | 7 | 33 | 8 | 26 | 9 | 18 |
| P.M. | 7 | 1 | 7 | 21 | 6 | 25 | 6 | 30 |

TABLE XIII.—Showing the Hours of the Morning and Evening when the Mean Temperature of Summer and Winter occurs in each season, as deduced by Table VIII.

| Mean of 1834 and 1834. | Summer. | | Winter. | |
|---|---|---|---|---|
| | h | m | h | m |
| A.M. | 7 | 48 | 8 | 50 |
| P.M. | 7 | 9 | 6 | 37 |

*Relation of the Mean Temperature of each Hour, and each similar pair of Hours, to the Mean Annual Temperature, or Mean Temperature of the whole Twenty-four Hours, as deduced by* Table VI.

It appears from the Hourly Registers at Plymouth and at Leith, that any number of daily observations of the thermometer seldom give a correct mean result without corresponding observations at night. The deviation with three observations in the day, is nearly three times as great as that arising from only two observations taken at any similar pair of hours. We gain, therefore, very little by multiplying daily thermometric observations without reference to some general principle of guidance. Hence the importance of determining, by experiment in different places, the relation between the mean temperature of the twenty-four hours and that of any single hour, or any similar pair of hours. In the following table will be found the mean temperature of each hour for the years 1833 and 1834, with the deviations of each from the mean of the whole year, or of the twenty-four hours, as deduced by Tables II. and IV., as also that of the mean temperature and deviations of each hour for the two years together, as deduced by Table VI.

**Table XIV.—Showing the relation between the Mean Temperature of each hour and that of the whole day.**

| | | Hours. | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 | 11 | 12 |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 1833. By Table II. | A.M. | Temp. | 48·80 | 48·52 | 48·34 | 48·14 | 48·14 | 48·85 | 50·18 | 52·14 | 53·67 | 55·14 | 56·15 | 56·95 |
| | | Deviation. | −3·33 | −3·61 | −3·79 | −3·99 | −3·99 | −3·28 | −1·95 | +0·01 | +1·54 | +3·01 | +4·02 | +4·82 |
| | P.M. | Temp. | 57·15 | 56·86 | 56·22 | 55·47 | 54·32 | 53·20 | 52·16 | 51·38 | 50·70 | 50·00 | 49·65 | 49·17 |
| | | Deviation | +5·02 | +4·72 | +4·09 | +3·34 | +2·19 | +1·07 | +0·03 | −0·75 | −1·43 | −2·13 | −2·48 | −2·96 |
| 1834. By Table IV. | A.M. | Temp. | 50·61 | 50·26 | 49·98 | 49·93 | 49·88 | 50·45 | 51·78 | 53·29 | 54·99 | 56·39 | 57·53 | 58·39 |
| | | Deviation | −3·03 | −3·41 | −3·69 | −3·74 | −3·79 | −3·22 | −1·89 | −0·38 | +1·32 | +2·72 | +3·86 | +4·72 |
| | P.M. | Temp. | 58·85 | 58·44 | 57·86 | 56·78 | 55·72 | 54·69 | 53·64 | 52·72 | 52·21 | 51·64 | 51·27 | 50·95 |
| | | Deviation | +5·18 | +4·77 | +4·19 | +3·11 | +2·05 | +1·02 | −0·03 | −0·95 | −1·46 | −2·03 | −2·40 | −2·72 |
| Mean by Table VI. | A.M. | Temp. | 49·72 | 49·39 | 49·16 | 49·03 | 49·00 | 49·65 | 50·98 | 52·63 | 54·33 | 55·76 | 56·84 | 57·67 |
| | | Deviation | −3·18 | −3·51 | −3·74 | −3·87 | −3·90 | −3·25 | −1·92 | −0·27 | +1·43 | +2·86 | +3·91 | +4·77 |
| | P.M. | Temp. | 58·00 | 57·68 | 57·04 | 56·12 | 55·02 | 53·91 | 52·90 | 52·05 | 51·45 | 50·85 | 50·54 | 50·11 |
| | | Deviation | +5·10 | +4·78 | +4·11 | +3·22 | +2·12 | +1·04 | +0·00 | −0·85 | −1·45 | −2·05 | −2·36 | −2·79 |

We perceive by this Table that the mean annual temperature of any hour in the day never differs more than about 5° from the mean annual temperature of the twenty-four hours. Sir David Brewster found the difference at Leith much less, viz. about 3° only. He also observed that of the two years 1824 and 1825, the deviations were uniformly greater in the warmer year, that is, in 1825, the mean temperature of which exceeded that of 1824 by more than 2°. A similar result, however, is not apparent at Plymouth, the deviations being less in the warmer year, that is, in 1834, the mean temperature of which exceeded that of 1825 by about a degree and a half: future observations may probably lead to some explanation of the cause of this anomaly.

The mean temperature for Plymouth may be, however, deduced from a register containing only one observation in the day, by applying, according to its sign, the correction given in the preceding table. Thus if the observed mean temperature for 9 A.M. as deduced by the observations through the whole year, was 54·33; then the mean temperature of the whole twenty-four hours for the year would be 54·33−1·43, since the mean temperature of 9 A.M. exceeds the mean temperature of the twenty-four hours by 1·43; that is, in applying the respective corrections we reverse the signs.

Table XV.—Showing the relation of the Mean Temperature of every similar Pair of Hours to that of the whole day, or Mean Annual Temperature.

| | Pair of Hours. | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 | 11 | 12 |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 1833. | Mean Temp. | 52·97 | 52·69 | 52·28 | 51·80 | 51·23 | 51·03 | 51·17 | 51·76 | 52·18 | 52·57 | 52.90 | 53·06 |
| | Deviation | +0·84 | +0·56 | +0·15 | −0·33 | −0·90 | -1·10 | −0·96 | −0·37 | −0·05 | +0·41 | +0·77 | +0·93 |
| 1834. | Mean Temp. | 54·74 | 54·35 | 53·92 | 53·35 | 52·80 | 52·57 | 52·70 | 53·00 | 53·60 | 54·01 | 54·40 | 54·67 |
| | Deviation | +1·07 | +0·68 | +0·25 | −0·32 | −0·87 | −1·20 | −0·97 | −0·67 | −0·07 | +0·34 | +0·73 | +1·00 |
| Mean | Mean Temp. | 53·86 | 53·53 | 53·10 | 52·57 | 52·01 | 51·79 | 51·94 | 52·34 | 52·88 | 53·30 | 53·69 | 53·89 |
| | Deviation | +0·96 | +0·63 | +0·20 | −0·33 | −0·89 | −1·11 | −0·96 | −0·66 | −0·02 | +0·40 | +0·79 | +0·99 |

It appears by this table—

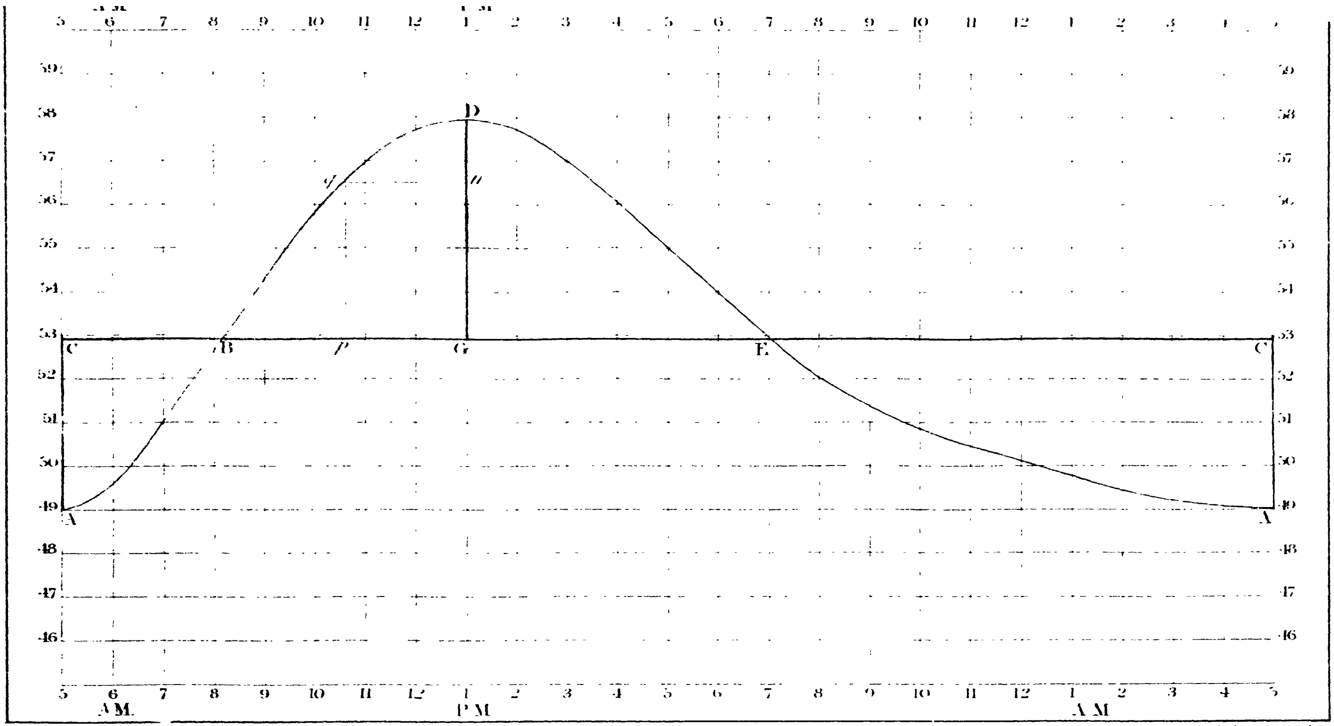
1st, That the deviation of any similar pair of hours from the temperature of the whole twenty-four or mean annual temperature does not greatly exceed one degree of Fahrenheit:

2ndly, That the mean temperature of the pairs of hours from $10^h$ to $3^h$ inclusive exceeds the mean annual temperature, or mean temperature of the whole twenty-four; and the mean temperature of the pairs of hours from $3^h$ to $10^h$ exclusive is less than the mean temperature of the whole twenty-four:

3rdly, That the mean temperature of $9^h$ and $9^h$ does not greatly differ from the mean annual temperature or mean temperature of the twenty-four hours.

We may infer from these results that the mean temperature of Plymouth may be very nearly determined by two observations in the day taken throughout the year at 9 A.M. and at 9 P.M; or by observations at any other similar pair of hours, provided we apply the correction for deviation as given in the preceding Table XV. according to its sign.

It may not be unworthy of remark, that the hours of $9^h$ and $9^h$ are nearly those which give the mean temperature of the day at Paris. At Leith, the mean temperature of $4^h$ and $4^h$ approaches nearest the mean, being those nearest the hours of the maximum and minimum.



A.M.
P.M.
A.M.
5 6 7 8 9 10 11 12 1 2 3 4 5 6 7 8 9 10 11 12 1 2 3 4 5
59 58 57 56 55 54 53 52 51 50 49 48 47 46
D
q
n
C
B
p
G
E
A

*Form of the four different branches observable in the mean annual daily curve.*

The preceding investigations furnish us with the following elements of the mean annual curve of the 24 hours.

| | |
|---|---|
| Mean temperature of the 24 hours by Table VI. | 52·90 |
| Mean minimum | 49 |
| Mean maximum | 58 |
| Difference between max. and min. | 9 |
| Difference between max. and mean temp. | 6·9 + |
| Difference between min. and mean temp | 3·9 − |

| | h m |
|---|---|
| Time of minimum temperature | 5 0 A.M. |
| Time of morning mean temperature | 8 9 P.M. |
| Interval between min. and morning mean | 3 9 |
| Time of maximum temperature | 1 0 P.M. |
| Interval between the morning mean and following max. | 4 51 |
| Time of evening mean temperature | 7 0 |
| Interval between the max. and evening mean | 6 0 |
| Interval between the morning and evening mean | 10 51 |
| Interval between the evening mean and following min. | 10 0 |
| Interval between the min. and following max. | 8 0 |
| Interval between the max. and following min. | 16 0 |

The mean annual curve of the 24 hours, beginning and ending with the point of minimum temperature is projected in Plate X. It may be divided into the four following branches, C C being the line of mean temperature:

1. The morning branch A B ascending from the minimum to the line of mean temperature, and including an interval of $3^h\ 9^m$;
2. The noon branch B D ascending from the mean temperature line to the maximum, and including an interval of $4^h\ 51^m$;
3. The afternoon branch D E descending from the maximum to the line of mean temperature, and including an interval of $6^h$;
4. The night branch E A descending from the line of mean temperature to the minimum, and including an interval of $10^h$.

On examining these four branches of the mean annual curve of the 24 hours at Leith, we may observe in them a somewhat close approximation to the parabola, on which account I have been led to calculate the mean annual hourly temperature for Plymouth, on the supposition that they may be represented by the abscissæ of parabolas. Taking C C in Plate X. as the line of mean temperature, then C A, G D, and their parallels may be considered as the abscissæ of the respective curvilinear branches A B, B D, &c. above mentioned. The

abscissæ referred to the line C C as the general line of the ordinates represent the deviations of the temperature of any point $q$ for any time $p$ taken on that line. The ordinates C B, B G, G E, E C make up the mean temperature line of 24 hours, or 1440 minutes. One degree of temperature in the projection is convertible into 60 minutes, being equal to an hour of time. We may hence readily obtain the abscissæ in terms of the ordinates or reciprocally, by merely converting degrees into time.

It is not therefore difficult from the known equation to the parabola, or $y^2 = 4ax$, to calculate the temperature of any point $q$ which should arise at any point of time $p$, on the supposition that the four curvilinear branches are semi-parabolas, and so compare the observed temperatures with those of calculation. For this purpose the following formulæ may be employed, which are easily arrived at.

Let T = the temp. for any point $q$ at any time $p$; $m$ = the min. = 49°; M = max. = 58°; $y$ = any ordinate taken on the line C C = variable in time.

Then for the morning branch A B we have $T = m + \frac{AC \times y^2}{(CB)^2}$;

for the noon branch B D......... $T = M - \frac{PG \times y^2}{(BG)^2}$;

for the afternoon branch D E ..... $T = M - \frac{DG \times y^2}{(GE)^2}$;

for the night branch E A......... $T = m + \frac{AC \times y^2}{(CE)^2}$.*

By these formulæ the curvilinear branches A B, B D, of the

* These formulæ, as shown by Sir David Brewster in his investigation of the Leith observations, are deducible in the following way:

Taking one of the branches, as B D, Plate X. we have by the property of the parabola $GD \propto (BG)^2$. If therefore $q$ be any point of temperature at any given point of time $p$, then in drawing $p\,q$ and $n\,q$

$$DG : Dn :: (GB)^2 : (nq)^2 : (y)^2 \text{: hence } Dn = \frac{GD \cdot y^2}{(GB)^2}.$$

Now it is to be observed that G D is the deviation of the maximum = M from the line of mean temp. = $\mu$, and $p\,q = Gn = (GD - nD)$ the deviation at time $p$; hence temp. at point D = $(\mu + GD)$ = M, and for any other point $q$ at any other time $p$ we have $T = (\mu + GD) - Dn$. Substituting therefore the values of $(\mu + GD)$ and $Dn$ in this equation, we get for the branch B D,

$$T = M - \frac{GD \cdot y^2}{(GB)}\text{; and for D E } T = M - \frac{DG \cdot y^2}{(GE)^2}.$$

By a similar process we arrive at the formulæ for the branches below the line of mean temperature C C in substituting $m$ for M, the value of $m$ being $m = (\mu - AC)$.

mean annual daily curve have been calculated, on the supposition that they are semi-parabolas of the following dimensions :

| | | |
|---|---|---|
| Morning branch A B. . | Ordinate B C = 192′ = 3°·2 | Absciss. A C = 234′ = 3°·9 |
| Noon branch B D . . . . | Ordinate B G = 288′ = 4°·8 | Absciss. G D = 306′ = 5°·1 |
| Afternoon branch D E | Ordinate G E = 360′ = 6° | Absciss. G D = 306 = 5°·1 |
| Night branch E A. . . . | Ordinate E C = 600′ = 10° | Absciss. A C = 234′ = 3°·9 |

The results are given in the following Table, which contains the calculated and observed temperature for each hour, together with the differences.

TABLE XVI.—Showing the mean annual Hourly Temperatures for 1833 and 1834 at Plymouth, as observed and calculated on the supposition that they may be represented by Parabolic Abscissæ.

| | Hours. | Observed Temp. | Calculted Temp. | Differences. |
|---|---|---|---|---|
| Morn. Branch A B. | 5 A. M. | 49° = $m$ | 49· | 0·00 |
| | 6 | 49·51 | 49·38 | + 0·13 |
| | 7 | 50·98 | 50·52 | + 0·46 |
| | 8 | 52·52 | 52·41 | + 0·11 |
| | 8 9m | 52·90 = $\mu$ | 52·89 | 0·00 |
| NoonBranch B D. | 9 | 54·33 | 54·36 | — 0·03 |
| | 10 | 55·76 | 56·03 | — 0·27 |
| | 11 | 56·84 | 57.12 | — 0·38 |
| | 12 P. M. | 57·67 | 57·78 | — 0·11 |
| | 1 | 58· = M | 58· | 0·00 |
| Aftern. Branch D E. | 2 | 57·65 | 57·85 | — 0·20 |
| | 3 | 57·04 | 57·47 | — 0·43 |
| | 4 | 56·12 | 56·80 | — 0·68 |
| | 5 | 55·02 | 55·85 | — 0·83 |
| | 6 | 53·94 | 54·64 | — 0·70 |
| | 7 | 52·90= $\mu$ | 53·90 | 0·00 |
| Night Branch E A. | 8 | 52 05 | 52·24 | — 0·19 |
| | 9 | 51·44 | 51·57 | — 0·13 |
| | 10 | 50 85 | 50·96 | — 0·11 |
| | 11 | 50·34 | 50·44 | — 0·10 |
| | 12 | 50·11 | 50·00 | + 0·11 |
| | 1 A. M. | 49·72 | 49·64 | + 0·08 |
| | 2 | 49·39 | 49·36 | + 0·03 |
| | 3 | 49·16 | 49.16 | 0·00 |
| | 4 | 49·03 | 49·04 | — 0 01 |

We observe in this Table that the approach of the branch E A at night to a semi-parabola is very close, the differences not being in any case two tenths of a degree of Fahrenheit. In the morning branch A B and noon branch B D the differences are somewhat more considerable, but still not so great as to destroy the approximation. In the afternoon branch, however, D E the deviations amount in one instance to more than eight tenths of a degree, and are too considerable to warrant this branch being taken as a semi-parabola. Whether more extended observations will tend to reconcile these differences is yet to be seen.

It is not unworthy of remark that although in the results of Dr. Brewster's inquiries the deviations did not in any case exceed a quarter of a degree of Fahrenheit, yet the differences, as at Plymouth, were greatest in the afternoon branch.

I hope at no distant period to present to the Association the results of the hourly observations for five complete years, after which it is intended that the register shall close. This Report, at present necessarily limited, may then probably admit of further extension and correction, so as to obtain better approximations than those arising from two years observations only.

Plymouth, July 20th, 1835.

## *Report of the Committee on Chemical Notation.*

Dr. Turner, the Chairman of the Committee appointed to take into consideration the adoption of an uniform system of chemical notation, made a report to the following effect:—

1st. That the majority of the Committee concur in approving of the employment of that system of notation which is already in general use on the Continent, though there exist among them some differences of opinion on points of detail.

2ndly. That they think it desirable not to deviate in the manner of notation from algebraic usage except so far as convenience requires.

3rdly. That they are of opinion that it would save much confusion if every chemist would always state explicitly the exact *quantities* which he intends to represent by his symbols.

Dr. Dalton stated to the Chemical Section his reasons for preferring the symbols which he had himself used from the commencement of the atomic theory in 1803 to the Berzelian system of notation subsequently introduced. In his opinion regard must be had to the arrangement and equilibrium of the atoms (especially elastic atoms) in every compound atom, as well as to their number and weights. A system either of *arrangements* without *weights*, or of *weights* without *arrangements*, he considered only half of what it should be.

*On the Infraorbital Cavities in Deers and Antelopes, called* Larmiers *by the older French Naturalists. By* A. Jacob, *M.D., Professor of Anatomy to the Royal College of Surgeons in Ireland.*

In compliance with the recommendation of the Committee of the Zoological Section of the Association made at the meeting in Cambridge in 1833, I have availed myself of such opportunities as have been afforded me for investigating the nature, structure, and uses of these remarkable parts. To those altogether unacquainted with the subject, it is necessary to state that they consist of two oval depressions about an inch and a half long, half an inch wide, and more than three quarters of an inch deep, in the majority of instances; situated on the side of the face, and so near to the inner angle of the eye that they create a very reasonable suspicion that they are connected with that organ, and hence the term *larmier* applied to them. The bottom of the depression is in most cases naked, but in some it is covered with the hair; consequently it is composed of the skin formed into an open sac accommodated in a corresponding depression in the bones of the face. In many animals provided with this organ a gutter, formed by folds of skin, leads so directly to it from the surface of the eye, that the passage of the tears from the one place to the other appears inevitable, while in others this communication is so imperfect that a doubt is at once raised as to its destination to such a purpose. If the part in question be not a cavity, as suggested by some, in which the overflowing secretions from the surface of the eye are disposed of by evaporation, another reason for its existence must be assigned. The arguments which may be urged against the supposition that it is destined to receive the tears are, first, that it exists in the antelopes and deers only, and is even absent or merely rudimental in many of these, while in animals said to be destitute of the usual canals for carrying off the tears to the nose, as the elephant and hippopotamus, it is absent; secondly, that the solid concretion generally found in it is not composed of such ingredients as the tears and other secretions from the surface of the eye should afford.

If the conclusion that these are cavities for the reception of tears be discarded, their identity of nature and character with the numerous provisions for the secretion of peculiar or odoriferous materials suggests itself. In many instances, especially

in the Mammalia, glands are found opening on the surface of the skin and pouring out peculiar fluids sometimes altogether unconnected with any organ. Such are the glands on the side of the head between the eye and ear of the elephant; those described by Tiedemann between the eye and nose in certain bats, consisting of a sac with a folded lining membrane, affording a fœtid oily secretion, and beneath the eye in the marmot and two-toed anteater; such also are the glands on the side of the chest of the shrew, described by St. Hilaire, and the inguinal glands of hares. Still more remarkable examples are furnised by the pouches affording the valuable odoriferous materials in the musk, beaver, and civet, and if additional examples be required they are found in the otter, mole, hyena, ichneumon, badger, and the dorsal gland in the peccary. That the cavities alluded to in the deers and antelopes afford peculiar and often odoriferous secretions, is established on the authority of several naturalists. Buffon describes the contents in the stag as resembling ear-wax. Daubenton found the secretion in an old stag so much indurated as to constitute a solid mass, or bezoar as he calls it, eleven lines long, seven broad, and six thick. Camper found pretty large hard yellowish particles in the fallow-deer. In a species of antelope first described by Dr. Herman Grimm, this organ secretes a fluid of such peculiar and distinct character that no doubt can be entertained of its nature. He describes it to be a yellowish fatty and viscous humour, having an odour between musk and castor; Vosmaer says that it hardens and becomes black in time, and that the animal rubs it off on the rails of its cage, but he could not detect the musky odour; Pallas, who describes the *Antilope Grimmea* particularly, concurs in these observations.

It may be objected to the conclusion that these are organs for the production of an odoriferous secretion, that the sac exhibits so little of glandular character that it appears inadequate for the purpose, especially when several of the external openings alluded to, as that on the head of the elephant and the back of the peccary, are merely the outlets of considerable glands; but, on the other hand, many organs of this character are mere sacs, as that on the face of the bats, the bottom of which presents a peculiar folded appearance, and the cavities in the musk and beaver, which afford the odoriferous secretion in such large quantity.

A statement respecting these infraorbital cavities has been made by the Rev. Gilbert White in his *Natural History of Selburne* which might appear to originate in some error, were it not supported by the more recent testimony of Major Hamilton

Smith. These gentlemen state that when the deer drinks, the air is forced out through these cavities, and according to Major Hamilton Smith may be felt by the hand, and affects the flame of a candle, when held to it. Notwithstanding such a positive statement by two observers of established character for faithful description, the passage of air through these cavities cannot take place; they are perfectly impervious toward the nostril: but I have no doubt that the fact stated is correct. The air seen to escape passes, not through the infraorbital sacs, but through the lachrymal passages, which are very large, consisting of two openings capable of admitting the end of a crow's quill, the entrance to a tortuous canal which conducts the tears to the extremity of the nose. Introducing a pipe into the outlet of the nasal duct at the extremity of the nose, I can without difficulty force a current of air or water through the nasal duct, and it therefore appears reasonable to admit that the effect observed by the two gentlemen alluded to arose from the animal forcing the air into the nostrils while the mouth and nose were immersed in water. Even in the human subject, air may be forced up the nasal duct into the lachrymal sac by filling the cavities of the nose from the lungs while the nostrils are closed by the hand.

Persons following up this investigation should be aware that these sinuses exist in a very imperfect state in many species, being in fact merely rudimental and incapable of affording the secretion which they are destined to provide in others. The last traces of the organ may even be detected in goats, sheep, and perhaps all the ruminants. It is a beautiful example of that adherence to an original type or model which is so conspicuous in animal organization, and as if in obedience to a law, that all the ruminants should be provided with a sinus beneath the eye for the secretion of a peculiar odoriferous matter, but that it should remain in an imperfect or unfinished state in those who do not require such additional aid to distinguish sex or recognise species.

The authorities quoted are *Buffon* in the original 4to edition, tom. vi. and *Suppl.* tom. iii.; Pallas, *Spicilegia Zoologica*; *White's Natural History of Selburne*; the supplementary volume of Griffith's Translation of Cuvier *On the Ruminants*, by Major Charles Hamilton Smith; and Camper, *Œuvres*, tom. i.

## *On the Effects of Acrid Poisons.* *By* Thomas Hodgkin, *M.D.*

The British Association for the Promotion of Science having requested Dr. Roupell and myself to prepare a description, accompanied with delineations, of the effects of acrid poisons, we have both of us been desirous of complying with the wishes of the Society, but circumstances have retarded the production of our Report. Our researches have for mutual convenience been conducted separately, but the results have been submitted to and approved by each of us. A part of the Report now printed was presented to the Medical Section at the meeting in Edinburgh; the remainder was communicated to the Session in Dublin.

The object which the Association had more particularly in view in calling for this report was, I conceive, to facilitate the recognition of the effects of acrid poisons, with a view to aid in judicial inquiries of a very serious nature, and also to obtain a contribution to our knowledge of pathological anatomy, on a point which, though it has engaged the special attention of several able and acute observers, still demands further elucidation, viz. the pathological appearances of the mucous membrane of the alimentary canal. As a preparation for an exact knowledge of the appearances which may be produced in the mucous membrane of this canal by acrid poisons and other irritants, a correct knowledge of this membrane in its healthy state is essential; but here, on the very threshold of our injury we are met with a serious difficulty common to it, and every attempt to elucidate the morbid anatomy of the alimentary canal, the want of this accurate and definite knowledge of the different parts of the canal and of the different appearances which may be presented by each part within the limits compatible with health. If any proof of this assertion were wanting it might be found in the various statements made by anatomists respecting the colour of the mucous membrane in its healthy state. The youthful but able pathologist Billard, whose premature death has deprived our profession of one of its most promising cultivators, devoted great pains to the elucidation of this subject, and has pointed out the differences which arise when digestion is actually going forward and when it is not. Other differences doubtless proceed from circumstances connected with the mode of death, even when it has influenced the stomach merely indirectly. A wide range of appearances depends on this single cause. The blood may leave the vessels of the stomach,

and its lining membrane may be nearly or quite white; or, on the other hand, they may be turgid by *cadaveric* congestion, and the lining membrane may be more intensely injected than in many cases of poisoning. Such differences though great are not more remarkable than those which are seen after death in the common integuments. It is however of the utmost importance that they should be well understood, since a mistake respecting them might seriously affect the reputation, if not the life, of a fellow-creature. The medical profession is greatly indebted to Dr. Yellowly, who long since called the attention of his brethren to this subject. The colour of the mucous membrane of the stomach is also very liable to be modified by its contents, which act on the blood in its vessels by transudation after death. This and some other circumstances to be hereafter mentioned have probably been the means of turning aside from the truth many able pathologists who have written on gastro-intestinal irritation, and more especially on chronic inflammation of the mucous membrane of the stomach. The researches of Dr. Stevens respecting the influence of different agents on the colour of the blood are well deserving attention in connexion with this subject, and appear to me to have thrown new light upon it. The colour of the stomach as well as many other parts may be greatly altered by exposure to the air after removal from the body.

The form and texture of the mucous coat of the stomach, which are of equal importance whether we regard the effects of disease or of poisons, appear to be involved in no less difficulty than the subject of the colour.

As a preliminary step to the right understanding of these alterations of texture we ought to be acquainted with those differences which depend in some degree on individual peculiarity, since the internal as well as the external teguments may admit of varieties of this kind. Still greater and more important differences are doubtless to be referred to the kind of diet which has been habitually employed; here however an almost insurmountable difficulty presents itself, since the diet employed in this and in most other civilized countries is of so various, and at the same time of so mixed a character as to render it almost impossible to connect cause and effect with any degree of certainty. If the subject were not neglected, as appears to have been altogether the case, some clue might possibly be found in the observation and collection of extreme cases which from time to time present themselves, and some assistance might be derived in the way of analogy from experiments performed on inferior animals fed expressly for this purpose.

Some of the differences of form and texture which come under

our observation in *cadaveric* inspections must be referred to alterations which the texture undergoes after death, and these may be of two kinds, either occasioned by its own molecular changes or by the action of the contents of the stomach, which not merely alter the colour of the fluids in the vessels, as I have before stated, but materially affect the form and texture of the membrane with which they are in contact. Extreme cases of this kind have long since been pointed out by John Hunter as cases of the digestion of the stomach by its own secretion. Short of this extreme effect there are many proceeding to a less considerable extent which must not be overlooked. The difficulties which I have enumerated are still further increased by an imperfect knowledge of the structure of the lining membrane of the stomach. The surface of the mucous membrane of the stomach is generally described as villous, and even Billard appears to agree in this description of it. I have at least a doubt respecting the accuracy of this statement. To me the surface of the stomach from which its secretion has been carefully removed and its place supplied by a little clear transparent water, appears to be indeed by no means perfectly smooth, yet not to be strictly villous like the internal surface of the small intestines. It has an indeterminate character which it is extremely difficult to describe in words. In the serous membranes the assistance of a powerful microscope enables us to distinguish delicate fibres intimately interlaced; but when the mucous membrane of the stomach is thus examined I can only observe an amorphous semitransparent mass in which no structural arrangement can be distinguished; there is therefore little to be expected from this mode of examination. When the recent healthy membrane is immersed in clear water it becomes slightly thickened, but when gently pressed between the fingers it resumes its former thinness. This would seem to indicate that the water had penetrated a sort of areolar or spongy tissue, but had not intimately combined with it as with mucus itself or with some other aqueous secretion. Some idea of the texture of the mucous membrane of the stomach may be formed from the vessels which ramify through it, and which are liable from various causes to become injected and consequently visible. When this injection is neither intense nor universal, the vessels may be traced with the assistance of a lens or even with the naked eye. They exhibit a character which may not inaptly be styled dendritic, since they closely resemble the marks in Mocha stone to which mineralogists have applied this epithet. These injected capillaries in the mucous membrane of the stomach are neither so minute and delicate, nor have they so well-defined, even and clean an

outline as the vessels which we may see ramifying through parts having a more firm and definite texture, as, for example, beneath the surface of the serous membranes or in the completely formed and perfectly cellular membranous adhesions which inflammation is apt to superadd to them. In fact the vessels in the mucous membrane of the stomach to which I am alluding bear a very close resemblance to the early attempts at organization which we may perceive in the recent false membranes upon the surface of inflamed serous membranes before they have lost the character of coagulable lymph. The cause of the appearance in question seems to be the same in both instances. The imperfect vessels ramify through a soft and scarcely concrete substance, by which they are barely supported. They consequently become more dilated than the minute branches from which they proceed.

The mucous membrane of the stomach, even where not thrown into rugæ of greater or less extent by the action of what is usually called the muscular coat but to which I would give the name of contractile fibrous coat, seeing it is not composed of strictly muscular tissue*, is not perfectly even and level. When placed on a flat surface with its free surface uppermost, we may generally perceive very slight undulations of small extent and little elevation, such as at times to require a particular direction of the light to make them visible. The elevated spots do not appear to possess a very determinate arrangement. They are generally of an oblong figure and vary in size from that of linseed to that of rice. The intervening depressions are of less extent and often seem almost linear. I have been thus particular in attempting to describe the internal surface of the stomach, not merely because I shall have occasion to refer to the effects of poison on particular parts, but from a belief that some very able and laborious pathologists have been led to form erroneous opinions respecting certain appearances of this surface. This I conceive to be particularly the case with respect to the small elevations of which I have last spoken. They are much more distinct in some stomachs than in others, and when strongly marked they appear to constitute that state which Louis designates *mammilloné*, and which he regards as an advanced stage of inflammation. I had long been familiar with this appearance without knowing what precise value to assign to it, yet strongly doubting its necessarily inflammatory origin. I am now satisfied that it depends on the natural structure of this part of the organ; and that according to circumstances, of which it is important to

* See the Appendix to the translation of Edwards on the Influence of Physical Agents on Life, by Dr. Hodgkin and Dr. Fisher.

be aware, it may either be very conspicuous or all but imperceptible. This conclusion is drawn from the examination not of human stomachs only, but from that of different inferior animals, in which similar or closely analogous appearances are observable. They may be seen in the stomach of the dog, but the most conclusive evidence is perhaps to be drawn from that of the horse. The stomach of this animal, (as has been well stated by my friend Bracy Clark in one of the articles written by him for Rees's *Cyclopædia*,) though single, may be compared to the more compound stomachs of the ruminating animals. A large portion, consisting of nearly the whole of the cardiac third, is covered with a smooth but thick cuticle, continuous with that which lines the œsophagus. It is bounded by a thick, well-defined, elevated edge. The portion which succeeds to this and occupies the whole or greater part of the middle third, is void of cuticle, and differs very much according to the state of the animal at the time of death, and according to the length of time which may have elapsed between the death of the animal and the inspection of its stomach. It may be compared to the digesting stomach of the ruminants. The resemblance is the most manifest when the animal has been recently killed whilst the process of digestion was going forward. This part of the stomach is then seen to be best supplied with blood. The elevations in the mucous membrane to which I have been alluding as slightly marked in the human stomach, are here strongly marked and exhibit a manifest analogy with the honeycombed surface of the stomach of a ruminant animal, but on a small scale. A considerable quantity of thick mucus is poured out upon this surface, and seems to be the secretion of the membrane itself. A special follicular apparatus, if it exists, is so indistinct as to escape the most careful search. When the animal, though recently killed, has not been digesting at the time of death, the elevations in this part of the mucous membrane, though more strongly marked than in the human subject, do not so clearly present the analogy before spoken of, but are very similar in form and character to those which are met with in man. If the animal have been long dead, and the stomach have become completely collapsed and flaccid, the mucous membrane of the middle third of the stomach becomes so smooth that the irregularities in its surface are almost imperceptible. The injection of this part of the stomach in the two states last mentioned is liable to considerable variety, which I conceive must, like similar differences observable in the human stomach, be attributed to accidental causes. In some human stomachs examined at a very

early period after death the irregularity is such as to justify the appellation of *mammillonée* which Louis has applied to it, whilst in the flaccid and long dead, and even in the recent stomach provided the secretions of the organ have acted upon the lining membrane, every trace of it is nearly or quite obliterated. I have observed the former or strongly marked state in the stomach of young persons in whom the idea of a chronic gastritis was inadmissible. At the same time I would observe that differences in the visibility and permanence of this irregularity of the mucous membrane of the stomach are met with to a sufficient extentto induce me to believe that stomachs differ among themselves in this respect independently of their being recently dead or having been engaged in digestion at the time of death. When the irregularities in question are strongly and permanently marked they may be regarded as the result of a real hypertrophy, since the membrane is not only firmer but thicker. This hypertrophy may often result from the use of certain kinds of food, but it seems also to be induced by other causes which occasion a determination of blood to the stomach. Thus, I have repeatedly met with it in stomachs which have been the seat of long-standing ulceration, even in those parts which do not appear to have the least participated in that state. I have also seen it in several cases in which hæmoptysis had repeatedly taken place; and I observe in the last fasciculus published by my friend Dr. Carswell, the representation of a part of the stomach of a person who had laboured under this affection which tends to confirm the remark which I have now made.

The last third or pyloric portion of the horse's stomach, like the middle, presents a somewhat uneven surface, but the elevations are much less both in height and extent. In fact it readily assumes almost an even surface, it is generally paler, and the mucus which lubricates its surface is less adherent and tenacious. I have sometimes seen indications of a very distinct follicular apparatus in this part. The human stomach, like that of the horse, generally becomes much less distinctly granular or uneven towards the pyloric extremity, and indications of a follicular apparatus may sometimes be seen, though I confess they are generally very equivocal. As I have taken some pains to discover how the human stomach is circumstanced with respect to follicular appendages, and as the subject is one on which I am persuaded a diversity of opinion exists, I shall take the liberty of making a further digression in order to say a few words respecting it. By some the existence of follicles is denied, by others certain appearances are regarded as indications of follicles

which I conceive ought not to be regarded as really possessing this character. Of the former class are some foreign pathologists. My friend Dr. Carswell appears to regard those elevations which I have been describing, and which when in a state of hypertrophy give to the mucous membrane of the stomach the character which Louis has designated *mammillonée*, as the follicles of the stomach, and the red spots which he has accurately described as sometimes occupying their centres he regards as the orifices of these follicles. I am induced to take a different view of these reddened centres. The mucous membrane of the stomach appears itself to be fully adequate to the production of mucus. Its follicles (if it possess any) are probably designed to bestow some peculiar properties on the juices of the stomach. We may therefore expect to find their situation occasionally pointed out by indications of a peculiar secretion at particular parts; and it is the fact, that we actually meet with such differences in the stomach rather than the actual demonstration of a follicular structure, on which I ground the opinion, which I offer rather as matter of conjecture than of absolute conviction.

The mucous membrane of the stomach sometimes presents small scattered spots, varying in size from about a twentieth to a tenth of an inch in diameter, from which the mucous membrane appears to have been removed, leaving a clean defined but by no means an elevated edge. Such spots are sometimes the commencement of ulceration, by which the stomach is actually perforated. They cannot, therefore, be regarded as the consequence of cadaveric solution. Spots are sometimes found scattered over the mucous membrane of the stomach scarcely exceeding in size those last mentioned, having a very slight depression, and rendered conspicuous chiefly by their colour, which is either dark brown or blackish. They are evidently produced by ecchymosis, and the idea that they are connected with a follicular apparatus is supported by the occasional existence of similar spots in the small intestines, where I have supposed them to be connected with the solitary glands. There is another appearance which I have met with in the stomach, and which though probably in degree *cadaveric*, concurs with the two preceding appearances to support the view which I have taken. I have found the mucous membrane of the stomach removed in numerous scattered spots having a circular figure, and varying considerably both in diameter and depth. They resemble the appearance which I first noticed in not having elevated edges, vascular areola, or other indications of ordinary ulceration, but they differ from them in appearing

to be more decidedly the effect of destructive solution. The edges were somewhat reduced in thickness. In some instances the mucous membrane was only rendered thin, in others wholly removed, and in the most advanced, the submucous cellular membrane was more or less removed, and the subjacent coat exposed. The surrounding cellular membrane, however, retained its healthy character, and allowed the free movement of the mucous membrane upon the subjacent coat, furnishing an additional argument against the inflammatory origin of the appearance which I have been describing. The exposed cellular membrane was generally of a pale almost milk-white colour, resembling that which is met with in the more extensive softenings of the stomach by the action of its own secretion.

I conceive that the appearance which I have thus described must have been occasioned by the operation of the follicular secretion. It may have commenced before death, but the symptoms of the case did not seem to warrant this idea, unless it were almost during the agony. The only difficulty seems to consist in the solvent process being confined to such limited spots, but this may perhaps be accounted for by the dissolving follicular secretion existing only in small quantity, though of intense quality, and, perhaps, on the membrane in the neighbourhood of the follicles having undergone some change by which it was rendered more susceptible of the influence of their secretion.

Since I have been engaged in the experiments which the request of the British Association has rendered necessary, I have met in the course of one of them with some appearances in the stomach of a dog which I regard as still further evidence in favour of the view which I have been laying before you. A dog was poisoned with oxalic acid in order to observe the peculiarities produced by this agent. In addition to other appearances which I shall have hereafter to notice, I observed numerous minute opake white spots, which I could imagine to be nothing else than follicles which had become preternaturally conspicuous amidst the surrounding altered mucous membrane*. When we consider the great variety of appearances which the mucous membrane of the stomach may present independently

* I have since on two or three occasions met with the like appearance in the human stomach. The distribution of the minute whitish spots resembling that which was observed in the other appearances referred to a follicular apparatus; and the evident difference between these spots, and the granular or mammillar elevations before alluded to, and which were likewise strongly marked in one of these cases, afford considerable confirmation to the view which I have offered.

of the effect of any poisonous ingesta, and which are often quite as striking as those appearances which are met with when poisons have been known to be taken, we cannot but be sensible how fully medical men are justified when in cases of legal inquiry they hesitate to draw any positive conclusions from the state of the stomach itself, and lay the principal stress on the chemical analysis of its contents, as well as of that of the matter which has been rejected by vomiting, and of the articles of which those suspected of having been poisoned are known to have partaken. In this branch of inquiry great progress has been made of late years, and to no one in this country are we more indebted for it, than to Dr. Christison. Although it must not be expected that the report which the British Association has called for will throw that light on the morbid appearances produced by poisoning which will give to them a similar degree of certainty with that possessed by chemical analysis, yet I believe we may reasonably entertain the hope, that the various and multiplied experiments in which I know that my colleague on this occasion has been laboriously engaged will do much towards it. Some interesting conclusions appear to me to be pointed to, by the few instances of poisoning which have fallen under my own observation, as well as by the small number of experiments which I have as yet been able to make. These I will now proceed to lay before the Association, together with representations of the appearances observed, the fidelity of which does great credit to the artists by whom they have been produced. There are always painful feelings accompanying experiments on inferior animals, yet I trust that in making them we may be fully justified on principle, when the object in view promises to be an advantage to man, provided we are careful to seek the end in view with the least expense of life and with the least possible amount of suffering. I felt considerable difficulty in making choice of the animals to be the subject of these experiments, and have endeavoured as far as possible to take those lives which for other reasons it was either necessary or desirable to sacrifice. Another point to be kept in view in selecting the objects of experiments is, that the animals may be such, that the conclusions to be drawn from them may with a good degree of analogy be applied to man. Dogs have in general been selected for this purpose; and their size, their sufficient degree of tenacity of life, and their patience under suffering warrant this choice. I have made some attempts with cats, supernumerary and worthless animals of this species being more easily obtained than in the case of dogs; but their extraordinary tenacity of life and the readiness with which they

reject from the stomach whatever offends it, induced me, after having unsuccessfully attempted to poison four of them with arsenic, to desist from making them the subject of experiment. It appeared desirable to employ an herbivorous as well as a carnivorous animal, and for this purpose I selected the horse, as the most accessible animal of sufficient size; and though his stomach differs materially from that of man, I conceive that the choice has not been an useless one.

### *Of the Modus Operandi of Poisons.*

This subject having been made the object of very careful inquiry by my friends Dr. Addison and John Morgan, with the result of showing that the influence of poison depends rather on a power exerted through the medium of the nerves, or by sympathy, than on the contamination of the circulating fluids by absorption, I have not thought it necessary to direct my attention to it in the experiments which I have made. There was one point, however, which appeared to me to be worthy of attention in reference to the modus operandi when the stomach is the organ acted upon by poisons, viz., Are the effects produced to be attributed to the mere injury of the organ, connected as it is with the rest of the system by the most astonishing sympathy; or is the principal stress to be laid on the specific action of the poison? The fact that a number of persons have been killed by drinking boiling water, who have died exhibiting many of the symptoms of poisoning, shows that the lesion of the stomach without specific influence is a very adequate cause of speedy death: still I was desirous of ascertaining the degree and extent of the mischief induced by this cause compared with what takes place when a poison is employed; I therefore had three ounces of water nearly at the boiling-point thrown by means of a syringe into the stomach of a small and young dog. It was almost instantly returned nearly as clear as when received, and still at a high temperature. After having thus rejected the water, the dog exhibited so little symptoms of uneasiness that I almost suspected that little or no mischief had been inflicted; but in a short time he made efforts to vomit, and rejected a clear fluid somewhat frothy and mixed with a little coagulated secretion resembling lymph or slightly heated albumen; he continued to repeat similar efforts at various intervals, the matter rejected bearing the same character as before, but occasionally tinged with blood. He appeared at times to suffer inconvenience in his throat, but his sufferings did not seem to be very severe; they appeared, however, to be on the increase

rather than on the decline, and about three hours after the water had been given, I found him weak, inclined to remain quiet, and with the upper part of the abdomen remarkably swollen, whilst the lower was as much contracted. The efforts to vomit were less frequent. Though cheerful when noticed, he had become cool and languid. Judging that the lesion of the stomach had now arrived at its height, and that death was inevitable, I had the animal killed by a blow on the head. On examining the stomach and œsophagus they presented an appearance which has been well represented by C. J. Canton. The œsophagus was of bright red, but its cuticular lining was not detached; its parietes were very much thickened by infiltration with a colourless fluid, constituting true inflammatory œdema, and bearing considerable resemblance to œdema of the glottis which is seen in man, except with respect to the redness and injection, which in œdema of the glottis in man are often wanting. The stomach was more intensely reddened than the œsophagus. It was distended with a considerable quantity of transparent but ropy secretion, but its parietes were not much thickened. The redness was far more intense towards the cardiac extremity, where blood appeared to be extravasated as well as injected. Towards the pylorus the discolouration was comparatively trifling.

The situation of the most intense effect produced by the irritation of hot water tends to confirm some observations which I have had occasion to make in examining the stomachs of persons poisoned by sulphuric acid, and leads me to offer a few remarks on

*The Inferences to be drawn from the Situation of the principal Lesion of the Stomach in Poisoning.*

In the cases to which I have alluded, the principal action of the boiling water and sulphuric acid were observed in the greater curvature immediately opposite the orifice of the œsophagus, rather than precisely at the cardiac extremity, where, in other cases, the most intense injection is generally met with. The repeated occurrence of this fact induces me to suppose, that when an intensely active agent, like the two which I have mentioned, has been swallowed or forced into the stomach, it is, as it were, discharged against that part of the internal surface of the stomach which is immediately opposite the opening, and that upon this spot an almost instantaneous effect is produced, which is deeper and more intense than that which is afterwards produced on other parts of the mucous membrane, when the agent is diffused over them, lowered in its activity by the mucus, which is rapidly secreted, and which does not merely dilute the

noxious agent, but in some degree protects the membrane. The fact that the spot which I have now pointed out is not precisely that at which the highest degree of vascularity is generally met with, may induce us to regard the discovery of a morbid appearance at that spot as a ground of suspicion that some fluid capable of producing an immediate effect has been received into the stomach. Even when the noxious agents received into the stomach do not produce the immediate effect which I have noticed in the case of boiling water and sulphuric acid, some inferences may be drawn from the situation of the morbid appearances. If the poison have been taken in the solid form, as, for example, when arsenic has been taken in substance, strongly marked effects will be produced at those particular spots on which the poison has lain, whilst the intervening portions either escape, or exhibit much less striking effects. If, on the other hand, the poison be taken in solution, and be not sufficiently intense at once to destroy the power of the stomach, its effects will be found most conspicuous in those parts which, under other circumstances, are the most frequent seat of injection, namely, the cardiac extremity, or even the whole cardiac half and the summits of the rugæ. In fact, the inflammation of the stomach produced by an irritating poison in a fluid form, and not acting immediately as an escharotic, appears to resemble that which takes place in the mucous membrane of the alimentary canal when *no* poison has been taken. At least the principal difference appears to exist in the superior intensity of the appearances which are occasionally observed in cases of poisoning. It is perfectly consistent with this remark, that we not only find the rugæ of the stomach reddened, especially at their summits, but also the edges of the valvulæ conniventes most intensely injected when the effect of the poison is continued into the small intestines. In the horses which I have had poisoned the orifices of the biliary and pancreatic ducts, which are marked by slight projection on the internal surface of the duodenum, were similarly reddened. The wax model of the stomach of a horse poisoned by corrosive sublimate given in solution, exhibits in a well-marked degree the effects of a fluid acrid poison; it is also worthy of attention that it is not merely the summits of the larger elevations, such as the rugæ of the stomach and the projecting orifices of the ducts, which become conspicuous by their superior injection, the summits of those smaller elevations to which I have called particular attention in describing the character of the internal surface of the stomach sometimes become similarly distinguished.

There is one circumstance in connexion with the redness and

injection of the mucous membrane of the stomach which appears to me to be worthy of attention, as affording in some instances a ground of distinction between the effects of decided inflammation and mere congestion. As far as I am aware, it has never been particularly pointed out. When an intense and diffused inflammation of the mucous membrane has been excited, the membrane is liable to be not only reddened by injection and thickened by the afflux of fluids to it, but an interstitial deposit of lymph seems to take place, which produces the appearances of small irregular opake whitish spots in the substance of the membrane itself. I do not know that I can better describe the appearance which I wish to point out than by a simple comparison. In thinnish gruel, prepared with oatmeal, we have a translucent viscid fluid, through which small opake whitish particles are diffused. Let us suppose the translucent fluid to be coloured by lake, or some other suitable pigment which does not destroy translucence, and the appearance to which I allude may be readily conceived. In cases of simple congestion, such as are produced by affections of the heart or other causes disturbing the circulation, and in cases of great irritation without the deposition of lymph interstitially, we may have redness and injection to a great degree of intensity, but without the accompanying irregular opake spots in the substance of the mucous membrane.

The appearance which I have just described was very conspicuous in the stomach of a man who had poisoned himself with hydrocyanic acid. Of the strength and quantity of the poison which he had taken I am unable to speak. It produced speedy, but not immediate, death. The inspection was not made by myself, but the stomach very shortly after its removal from the body was brought to Guy's Hospital, and the appearances which it presented were carefully copied by the very accurate pencil of C. J. Canton. At the same time I must observe that the appearance in question is so intimately connected with the structure of the membrane as to render a perfect delineation almost impossible. Similar, but rather less conspicuous, interstitial opake spots were observed in the stomach of an elderly person who had taken arsenic. This stomach was, like the former, not met with in one of my own inspections, but was brought to me some hours after its removal by an able anatomist, who had conducted the examination. This circumstance, as well as that of the arsenic having been taken, as it was supposed, in fluid form, may account for the absence of some of the other appearances which are often, and perhaps

generally, seen in cases of poisoning by the swallowing of arsenic.

The character of the secretion upon the surface of the mucous membrane will sometimes throw considerable light on the condition of the membrane before death. In the case of the dog which had received boiling water, we have seen that a large quantity of fluid was secreted, since the stomach was found distended with it, and a considerable quantity had also been rejected by repeated vomiting. Not only the quantity but the quality of the secretion was altered, for besides the clear and glairy fluid, there was also some opake and partially coagulated matter, which appeared to consist of lymph. The fact that none of this was found in the stomach after death shows that it did not attach itself to the lining membrane in the form of a false membrane;—the abundance of the fluid secretion, combined with the continued and forcible action of the contractile fibrous coat, having probably been the cause which prevented its doing so. In other instances, when the irritating cause is very active and remains applied to particular spots, the secretion is rather lymph than mucus, and remains attached to the lining membrane, except under particular circumstances, which I shall have to notice in one of the cases I am about to relate.

The presence of a small quantity of blood in the matter secreted is equally worthy of attention with the production of lymph instead of ordinary mucus. In whatever way the escape of this blood is brought about, it is an evidence of the violence of the injury which the mucous membrane has received. It would appear, however, that it takes place in two modes, which deserve particular attention. In the one case the vessels seem to give way under the immediate influence of the violence which they receive, as well as from considerable and sudden injection. The hæmorrhage in this case resembles that which takes place from mechanical injury, or more closely that from the Schneiderian membrane which occasionally takes place under violent exertion. In the other case to which I allude, the escape of blood is the result of a more slow and gradual process. It appears to be brought about by the alteration of structure which takes place as the result of the inflammation which the irritating cause has created. The blood escapes from numerous minute points at which the redness is most intense, the substance of the membrane having become soft and tender, though somewhat thickened.

It is this softening of the texture, the result of inflammation,

and which prepares the way for the escape of blood at numerous points, which appears to me to be worthy of particular attention, since it seems quite analogous to that which takes place in acutely inflamed serous membranes when plastic lymph is thrown out and is about to become organized. In the case of the serous membranes, these numerous and minute extravasations of blood into the closely applied or adherent lymph appear to be the first stage by which the organization of the false membrane commences.

I shall now proceed to relate some of the cases and experiments which have furnished the opportunities of producing the drawings and models which I have to submit to the inspection of the Medical Section.

12—10*mo.*—1829. *Guy's Hospital.*—No. 1.—Examination of the body of William Robert Squires, æt. 16, admitted into Luke's Ward on the 11th, and who died about twenty-six hours after having swallowed arsenic by accident. It appears that on the morning of the 10th he picked up a piece of cheese which his master had charged with arsenic and placed as a poison for rats. Having shaken or blown it to get rid of the dust or flour which he thought was upon it, he swallowed it. He afterwards took his dinner and went to his work, but was seized with vomiting and tormina. The cause of his illness was not suspected until the following day, when the lad's master discovered that the poisoned cheese had been removed. He was brought to the hospital, and two 5-grain doses of sulphate of zinc were given. They produced vomiting of bilious matter, mixed with a flake or two of a substance resembling a semitransparent membrane, spotted with blood. A blister was then applied, but he died almost immediately after. His pulse was very quick, but his symptoms, even a short time before death, did not appear very urgent. He had passed stools, and had complained of pain of head, but not of heat of the throat.

The appearance of the body indicated an age less than that assigned to the lad. The body was in good condition, but mottled with rather light-coloured irregular livid spots.

Head.—The head was not opened.

Chest.—The viscera of the chest were healthy, but the lungs exhibited considerable cadaveric engorgement. The remains of the thymus gland were large. The heart was rather small and contracted. It contained some coagulated blood.

Abdomen.—There was a generally diffused light rose-colour over the greater part of the exterior of the intestines, but it appeared rather to receive the tinge from congestion than from inflammation, not being attended with any effusion of lymph or other product of inflammation, and not particularly affecting parts in contact, but portions of the whole calibre at intervals, which generally occurred in depending portions, were of a deeper colour than the rest. The interior of the œsophagus was to all appearance healthy, or at most of a *very faint* rose-colour. The mu-

cous membrane of the stomach was corrugated, and exhibited extensive deep and bright injection, not nearly so uniformly diffused as is often the case, but most considerably affecting the rugæ. The middle third was the most considerably affected, but there was no marked difference at that part which is opposed to the cardiac orifice. There was no decided abrasion, but at two or three small points the effused lymph was adherent. The pyloric extremity was the least reddened, but at this part the follicular glands were elevated and very distinct. The stomach contained a considerable quantity of watery bilious fluid, and a mass which appeared chiefly to consist of a coagulated secretion resembling the plastic lymph on the surface of an inflamed serous membrane. It was about the size of the palm of one's hand, and had very strongly received the impression of the rugæ of the stomach, and the surface in contact with the lining membrane closely resembled it in colour and in the distribution of the extravasated blood intimately intermixed with it on this surface. The other surface resembled common coagulable lymph, but entangled in it there was a fragment of what appeared to be partially dissolved cheese, mixed with numerous particles of white opake matter. A small quantity taken from this part, dried and mixed with black flux and heated, afforded a distinct trace of sublimed metallic arsenic. Another portion, reduced on charcoal before the blowpipe, yielded the alliaceous odour. There was a diffused and light but not bright redness of the duodenum. A similar condition, but in a much less marked degree, was observable throughout the small intestines, in which the solitary glands were particularly distinct; there was scarcely any fæcal matter in the canal, but there was abundance of secretion, which in its character appeared intermediate between ordinary mucus and coagulable lymph. There was a slight degree of œdema of the submucous cellular membrane. The large intestines were of a more natural appearance. The mucous membrane was generally pale, but there was a manifest increase of redness about the verge of the anus. Many of the mesenteric glands were much enlarged. The structure of the liver appeared to be healthy, with the exception of some scattered ecchymosed spots, obviously of recent formation, and a little dappling of a lighter colour. The gall-bladder was distended with rather dilute bile. The spleen and pancreas were healthy, as were also the kidneys and bladder, excepting some increased vascularity of the mucous membrane of the bladder near the cervix at the posterior part.

The points worthy of remark in this case appear to be: 1st, That though a considerable quantity of arsenic had been taken, the symptoms which followed were not proportionably urgent and rapid. For this there appeared to have been at least two causes. The cheese in which the arsenic was involved having resisted digestion, seems to have prevented much of the arsenic from coming in contact with the stomach. The food which was taken almost immediately after the swallowing of the poison may have also acted in a similar manner. It may also have had the effect of exciting the healthy action of the stomach by setting up the digestive process: this appears to be analogous to what takes place in horses which have eaten the leaves of the yew tree, which are

an active poison to horses and other cattle. They generally die in a few hours after taking this poison; but it has been shown by my friend Bracy Clark, that if food be taken in conjunction with, or immediately after, the yew leaves, the injurious effects do not follow, but the poison and the food appear to be digested together. The second point is the complete illustration of the remarks which I have offered respecting the production of coagulable lymph, and of the escape of blood from minute points on the inflamed surface.

3rdly. The detachment of this layer of lymph from the mucous surface, which was probably brought about by the efforts to vomit, renewed with increased energy by the emetics of sulphate of zinc. This is a practical point, bearing on the use of emetics and the mode of employing them *.

No. 2. This case occurred so recently as the 21st of last month, (August, 1834.) It is that of a middle-aged man who like the lad in the preceding case had taken arsenic.

22—8mo.—1834. *Guy's Hospital.*—Examination of the body of A. B., aged about 35 years, a patient of B. B. Cooper's in Accident Ward, admitted on the 21st, a short time after he had taken about an ounce of arsenic. He was a man of dissolute and intemperate habits and took the arsenic whilst in a state of intoxication. Vomiting had taken place in about half an hour after he had swallowed the poison. On medical assistance being obtained, the stomach-pump was freely employed; he was afterwards removed to the hospital, where an emetic of sulphate of zinc was administered and acted pretty freely. The patient was then perfectly sensible, and endeavoured as far as lay in his power to cooperate with the means employed for his recovery. Besides the emetic a considerable quantity of chalk was given to him. He was affected with purging as well as vomiting. His first stools were not seen, but those which he afterwards passed contained much jellylike mucus. He passed some urine, the character of which was not noticed. The abdomen was somewhat painful when pressed.

He sunk in a state of collapse about midnight.

The external appearances presented nothing remarkable. The body was in good condition as to flesh, and its surface generally pale.

The head was not opened.

The pleura on the right side was almost universally adherent by a firm old adventitious cellular membrane. The left was perfectly free from adhesions; there was little or no serum in its cavity. The substance of the lungs appeared generally crepitant and healthy, but posteriorly there was a good deal of sanguineous engorgement, having very much the character of pulmonary apoplexy. In the anterior portion of the lung were one or two rounded portions having completely this character. The pericardium contained some straw-coloured serum. The heart was large, but neither remarkably gorged nor contracted; the right auricle was rather distended; the blood in the right ventri-

* A drawing of the rejected portion of lymph illustrated this case.

cle was partially coagulated with some separation of fibrin. The peritoneum was partially minutely injected, especially towards the cardiac extremity of the stomach, and on some of the convolutions of the small intestines. The branches of the mesenteric veins were somewhat distended. There was a small quantity of straw-coloured serum in the lower pelvis, with some tender diaphanous films of coagulable lymph which retained the serum in its meshes. (This lymph may have separated from the serum by coagulation after death.) The stomach was flaccid and slightly distended, containing air and dirty turbid chocolate-colour fluid in which were some gritty matter, and softer whitish powder, probably chalk. There was no concrete mucus or lymph adherent to the internal surface of the stomach. The mucous membrane was generally of an intense red colour, deepest about the middle towards the smaller curvature, a little less so at the cardia, and considerably less towards the pylorus and greater curvature. The redness was not altogether diffused, but for the most part assumed the character of a dendritic capillary injection. In some instances this redness was most intense where rugæ appeared to have existed. Along the greater curvature and a little towards the pylorus the remains of the rugæ were very evident and of a livid or chocolate colour, the substance of the mucous membrane being considerably thickened along their course. The surface of the membrane generally was slightly granular; there was no appearance of abrasion produced either by the poison or the stomach-pump. The mucous membrane did not appear particularly soft, but was perhaps a little thickened. In the injected parts between the distended dendritic capillaries there was a small appearance of white opacity, suggesting the idea that a little lymph had been separated in the substance of the membrane. This appearance was less distinct than in some other cases of a similar kind. The duodenum was mottled with red colour, but not by any means intensely injected. Throughout the small intestines there was a marked redness approaching to lilac and of a light colour in the course of the valvulæ conniventes. The mucus which they contained was rather thick, grumous and turbid, but by no means ropy. The aggregate and solitary glands were not particularly developed. The mucous membrane of the colon as far as it was examined was pale and covered with thick mucus. Towards the rectum, and in that intestine, the mucous membrane was a little injected in spots; this was most considerable towards the anus. The mucous membrane at this part resembled paste, and had very little odour. The mucous glands were developed. The liver was rather large, of a mottled yellow colour, with a granular appearance, having a good deal of the character of liver met with after the abuse of mercury, the acini assuming the form of small rounded bodies: in some spots there were contraction and induration of the intervening substance, and one or two small semicartilaginous bodies imbedded in its substance near the surface; they were probably the effect of blows or some other old local injury. The gall-bladder was distended with greenish bile; no trace of bile had been observed in the alimentary canal. The pancreas was healthy, but perhaps more coloured than is usual. The spleen was of moderate size and apparently healthy. The kidneys were

healthy, but rather injected. The bladder was contracted, and its mucous membrane a little injected, especially towards the cervix, where the veins were distended.

The principal points of consideration which this case suggests are, 1st, the greater rapidity with which death followed the taking the poison; 2nd, the differences in the appearances observed after death, consisting in the more general diffusion of redness and injection, and in the absence of plastic lymph; 3rd, the different mode of treatment, consisting in the use of the stomach-pump and the liberal use of emetics, to which may be ascribed the removal of the coagulable lymph, had it been thrown out, and the application of arsenic in solution to almost every part of the stomach instead of partially in a solid form.

The contents of the stomach and small intestine of this patient were very carefully examined by R. H. Bretts, a pupil at Guy's Hospital, who has devoted great attention to chemical research. I need not detail the process to which he had recourse. There was no difficulty in the discovery of arsenic in the stomach, from which some remains of the white oxide were taken. The presence of the arsenic in the intestine was made certain, but not without considerable difficulty, and its quantity appeared to be very minute. On this I would lay some stress, as in the experiments which I have next to relate, the one on a dog, the other on a horse, no arsenic could be discovered in the intestines though carefully sought by equally practised analysts.

No. 3. The next example of poisoning by arsenic which I shall relate is that of a dog, and here I would observe that I met with considerable difficulty before I succeeded in having a dog killed with this poison; for although they do not at first refuse to take either liquid or solid food with which arsenic has been mixed, yet having taken it they readily reject it from the stomach, and then appear to grow suspicious and generally refuse further doses. I at length succeeded, with the assistance of T. Davis, by giving repeated doses, so small as to be disguised, at the intervals of an hour each to a hungry dog. He retained some of the doses for an hour and half or more. He vomited after each. He survived the first dose more than twelve hours; but as he died in the course of the night when he was not watched, it is impossible to state the exact time. In the stomach of this dog, which was examined the following morning, the mucous membrane was found deeply reddened towards the cardiac extremity and in other parts, to some of which the arsenic in substance was attached, being intermixed with the secretion, which in some respects resembled coagulable lymph and assumed the form of a false membrane. When this layer was fresh raised from the surface of the membrane the inflamed and reddened texture was of a bright colour. When the secretion had been previously separated, the membrane, coloured by injection or extravasation, presented a deeper hue. The viscid as well as more solid exudation from the surface of the stomach was somewhat tinged with blood, some of the particles of which were examined by my friend J. J. Lister; they had not wholly lost their form, but the regularity of their outline was considerably impaired.

Though this blood had doubtless escaped from some portion of abraded

surface, I did not discover any spot in which abrasion had taken place. Almost every part on the intestinal canal of this dog exhibited more or less injection of a bright colour. In the higher portions the redness occupied nearly the whole surface, but lower down it strikingly marked the summits of the rugæ. The mucous glands towards the termination of the rectum were considerably enlarged. The contents of the intestinal canal were examined by G. O. Rees, but no trace of arsenic could be detected even in the small intestines*.

No. 4. This illustration I take from the case of a horse which received 2½ ounces of arsenic rolled up in dry paper. In four hours the effect of the poison was strongly shown; and in the evening, ten hours from the time at which it was given, the animal died. The stomach was examined the following morning. It was distended with masticated hay, mixed with a moderate quantity of fluid. A considerable quantity of the arsenic in substance was found about the greater curvature rather more than one third from the pylorus, and consequently applied to the second and third portions of the mucous membrane. Traces of the arsenic were evident in many other parts of the stomach, although it was nowhere collected in substance as at the spot just mentioned. The greater portion of the mucous surface of the middle third was covered with a tenacious layer of secretion intermediate between lymph and mucus. It was nearly white when applied to the stomach, but the other surface was discoloured as well as roughened by the intermixed and adherent particles of food. The mucous membrane beneath this layer was deeply coloured with blood in those parts with which the arsenic appeared to have been in contact, whilst over a large surface in which this was not the case, the membrane, though not white, did not seem to be morbidly coloured. The summits of the rugæ and other prominent portions of the mucous surface, both in the third portion of the stomach and in the pylorus, were especially reddened. This was the case with the orifices of the biliary and pancreatic ducts. That portion of the stomach which is covered with a strong cuticular lining did not appear to be at all affected. Most of the bots, of which there were several in this stomach, were still alive. There was some redness in the course of the alimentary canal, but it was neither intense nor otherwise remarkable. The contents of these intestines were very carefully examined, but no arsenic was discovered.

No. 5. A second horse received a portion of arsenic in the same manner as the preceding, except that instead of being allowed to die he was killed in four hours, before he had betrayed any symptom of derangement from the dose which he had taken. The stomach was soon after examined. It contained about the same quantity of food as in the former case, and the arsenic in substance was found collected in precisely the same part of the stomach. A considerable portion, however, had also passed the pylorus. The appearances observed in this case were very similar to those observed in the preceding instance, but they were much less intense. The same kind of tenacious layer covered the greater

* Wax models by Joseph Towne illustrated this and the following cases.

part of the middle portion of the organ. Its free surface was discoloured with intermixed and attached particles of food, but the thickness of this layer was much less than in the former case. The membrane beneath it was but slightly discoloured, except where immediately in contact with the arsenic. That portion of the poison which had passed into the duodenum was implicated in a mass of coagulated lymph pretty firmly adherent to the surface of the membrane; on raising it, the under surface presented numerous bright red bloody points, and a similar appearance was seen on the membrane from which it had been detached. Lower down in the intestinal canal I did not discover anything remarkable. I must not omit to observe that in the stomach of the horse, where no poison had been taken, the viscid adherent mucous secretion is liable to be discoloured on its free surface by adherent particles of food, but a little careful attention will distinguish this layer from the more membranous character of that produced when arsenic has been given.

The 6th example which I shall bring forward is that of a horse poisoned with corrosive sublimate, which was given in solution in gruel. The symptoms in this case were at least as urgent as in the first case of poisoning with arsenic. I have already remarked some of the peculiarities distinguishing this form of poisoning from that in which a solid irritation is applied to the mucous membrane.

The 7th case is of a very different character from the six preceding, and appears to me to be worthy of particular attention. A pretty strong solution of oxalic acid, containing, I believe, rather more than a dram of the crystallized acid, was injected into the stomach of a dog as in the case of the boiling water. The effect was immediate, and death took place in about a quarter of an hour, with symptoms which I did not witness and cannot now relate. Death in this case was more speedy than I had anticipated, and I was consequently not prepared to examine the body for rather more than twelve hours after it had taken place. At the opening of the abdomen I was struck with the dryness of the peritoneum and the general paleness of the contained viscera. This was particularly the case with the intestinal canal. The fat of the epiploon and other parts within the abdomen was also remarkably firm and white. The cardiac extremity of the stomach was flaccid and exhibited a dingy colour even on its peritoneal surface. Internally the mucous membrane appeared partially removed, as if by solution, at and near this part. This and some other parts which were coloured were of a brown or slate colour, the other parts of the stomach were pale and partially translucent. I have already noticed the small opake white scattered points which I have been induced to regard as follicles. Towards the pylorus the mucus on the surface of the membrane was more abundant and opake; the intestinal canal was not only of a whitish colour, as I have before stated, but the intestines were unusually firm as if filled with a pretty stiff pultaceous substance. On opening it, the coat appeared greatly thickened, but on examination this appearance was found to be produced by a thick opake white secretion deposited on the mucous surface, and bearing some similarity to a very thick white fur on

the dorsum of a tongue. When this was removed the pale and almost unchanged villous membrane was distinctly visible. The membrane was perhaps a little softened. When the secretion just mentioned was not of an opake whitish colour, it was of a dusky brown of no great intensity. This colour was distinctly situated on the edges of the valvulæ conniventes, and was in all probability produced by the action of the acid on the colouring matter of the blood with which the edges of the valvulæ *conniventes* had been injected.

Not only the peculiar appearance which I have just described extended to all or to the greater part of the intestinal canal, but strong acid properties were manifested in it. This rapid diffusion of this noxious agent through so large a portion of the alimentary canal forms a striking contrast with those cases in which arsenic was the poison employed, in which, as it has already been stated, either no trace of the poison, or such only as were extremely faint, could be detected at more than a short distance beyond the pylorus, although the animal survived the administration of the poison for some hours. It would seem that this extent of the diffusion of the noxious agent is commonly the case with acid poisons and may be regarded as characteristic*.

The blood in the mesenteric veins was of a dark colour, confirming the observations of Dr. A. T. Thompson and Dr. Perry of Lausanne. It also appeared to possess acid properties. In observing the effect of oxalic acid on the stomach of the dog, as seen in this case, one can scarcely fail to be struck with the strong resemblance which it bears to the state of the human stomach as often seen in post mortem examinations, more especially with respect to the coloured and softened texture of the mucous membrane. The peculiarities in both of these respects have been strongly insisted upon as indicative of chronic inflammation. They unquestionably may be met with when this state has existed, but if I am not greatly mistaken they also occur when this has not been the case, and they may with much more probability be referred to the action of the juices of the stomach, which vary greatly in their properties, and doubtless act not only after death, but even in some degree before life is quite extinct.

No. 8. The last case which I have to bring forward is that of poisoning by spirits of wine. In investigating the action of poisons, it was next to impossible to lose sight of an agent, which not only involves many in inextricable misery, but hurries thousands to their graves.

Rather more than an ounce of strong spirit was injected into the stomach of a dog, as in the case of the experiment with boiling water. The effect was immediate. In a minute and a half he vomited mucus and a little blood; in three minutes he was wandering and falling in different directions; in five he fell down and voided a quantity of urine; the muscles of the abdomen and extremities were thrown into violent action;

* The mention of this fact to the Medical Section at the meeting in Edinburgh gave occasion to my friend Dr. William Thomson to show me a striking illustration of this principle in a representation of the effect of poisoning by nitric acid preserved in the splendid and extensive collection of pathological drawings in the possession of his father Dr. J. Thomson, Professor of Pathology.

in thirty-eight minutes he appeared to be dead, but he afterwards vomited a thick slimy fluid smelling strongly of alcohol, and died in forty-two minutes. Circumstances prevented the examination from taking place till the following day, when the stomach presented an appearance which is well represented by C. J. Canton. The mucous membrane of the stomach offered strongly marked and irregular rugæ in the intervals between which the mucous membrane had a corrugated appearance. It was universally of a reddish brown colour, which, however, was not universally intense. Since making this experiment I have learnt that strikingly similar effects were produced by the exhibition of strong spirit in an experiment performed by my friend and colleague Dr. Roupell, the result of which he has shown in the second of his splendid fasciculi. The brighter colour produced in Dr. Roupell's experiment is probably a more genuine effect of alcohol than the browner colour which I obtained, and which may have been in part occasioned by some cadaveric change. There can be little doubt that the extreme effect of ardent spirit in these cases, in which it acted as one of the most prompt of the acrid poisons, is only an exaggeration of that diffused and pernicious irritation of the mucous membrane of the stomach which spirit-drinkers are constantly keeping up or renewing.

## *On Acrid Poisons.* *By* G. L. Roupell, *M.D.*

A further Report on the subject of Poisons was submitted by Dr. Roupell to the Meeting. The object of the author was to advance a step in showing the mode of operation of poisonous substances. The labours of Dr. Hodgkin and Dr. Roupell had hitherto been confined to the description and illustration of appearances resulting from the direct application of poisons to the mucous membranes. Dr. Roupell next proceeded to ascertain the effect which poisons produce when introduced into the circulation, and he concluded his paper with some conjectures suggested by the facts presented to him as to the probable origin of certain forms of disease.

The mode adopted in the experiments about to be detailed was to inject various agents into the veins; some actively and intrinsically poisonous; others poisonous only by their chemical relation to the circulating fluids. The substances employed were arsenic, corrosive sublimate, tartarized antimony, muriate of iron, acetate of lead, and kreosote.

Two results were common to the employment of all. First, a fatal termination from the administration of large doses of each; and secondly, a complete absence of all symptoms of derangement from the employment of smaller doses even of the most virulent.

The paper was accompanied by several highly finished drawings; the first of which exhibited the œsophagus, the stomach, and part of the duodenum of a dog poisoned by the injection of arsenic into the veins. The œsophagus was natural in appearance. The stomach exhibited the hour-glass contraction and contained about an ounce of toughish mucus. The tips of some of the rugæ were reddened at the contracted part, but it differed little from health at either extremity. The mucous membrane of the small intestines was acutely inflamed, presenting narrow bands about two lines in breadth of a bright red colour extending transversely across the intestine, alternating with equal spaces of apparently sound membrane. This striped appearance was chiefly at the upper portion of the small intestines, the inflammation becoming more diffused in extent and diminishing in degree as it was traced downwards, and finally terminated at the extremity of this portion of the intestinal canal. The details of this experiment were as follow. An ounce of a saturated

solution of arsenic, made by boiling distilled water with an excess of arsenious acid and allowing it to cool, was thrown by means of a small syringe, at two o'clock P. M., into the femoral vein. For 3 minutes there was no obvious effect, but at the end of that time vomiting commenced, and a quantity of half-digested food was thrown off the stomach. The respiration then became hurried and the animal appeared faint. In 10 minutes there was great intestinal movement, the abdomen being frequently and forcibly drawn in. After 25 minutes, vomiting was renewed, paralysis of the hind legs came on, and the animal died in 65 minutes. Examination was made on the next day. The limbs were rigid, the blood was fluid, the lungs were collapsed and had a rosy tint, but were not inflamed. The peritoneum was reddish. The appearances of the stomach and small intestines have been already described. The large intestines contained solid fæcal matter and were quite free from morbid alteration. There was no apparent change in the mucous membrane of the trachæa or bronchi, none in the inner lining of the heart, veins, or arteries, none at least in their larger branches.

The points of interest in this experiment are, first, the absence of inflammation in so many tissues with which the poison must have come into contact; secondly, its action on the mucous membrane alone; thirdly, the predilection shown for the mucous membrane of the small intestines, and in this instance the limitation of its action to that part of the alimentary canal, where it was intense, the membrane being covered with a layer of tough mucus mixed with blood; fourthly, a circumstance frequently observed in cases of poison, the rigidity of the limbs, the blood remaining fluid.

A second drawing exhibited also the stomach and part of the duodenum of a dog poisoned with arsenic thrown into the veins. In this instance the mucous membrane both of the stomach and intestines was intensely red, the redness extending throughout the whole track of the intestinal tube. This experiment was a modification of the last, the difference consisting in the greater strength of the animal and a diminution in the quantity of the poison. An ounce of a saturated solution of arsenious acid was indeed here also employed, but the solution was filtered; a thin floating pellicle of arsenic was removed from its surface. The solution was thrown into the femoral vein at 12 minutes past 1. The animal vomited almost immediately after the operation. In 12 minutes solid fæcal matter was passed from the bowels followed by tenesmus; in 35 minutes dysentery was induced, and the animal made attempts at vomiting. These symptoms continued with more or less severity about 3 hours, when death took

place. Examination was made 22 hours after; the lungs were red throughout and gorged in patches. The stomach externally was very vascular. The whole of the mucous membrane from the cardiac orifice of the stomach to the extremity of the rectum was in the highest possible state of inflammation. The stomach contained about 4 ounces of a frothy mucus mixed with blood, and a small quantity of a similar secretion was found coating the intestines. The inner lining of the urinary organs was redder than natural. The lining membrane of the heart, large arteries and veins was in its healthy condition.

In this case a longer interval between the administration of the poison and death gave time for the establishment of inflammation in more situations, and allowed it to proceed to a greater extent in those parts which were irritated in the preceding experiment.

A third drawing exhibited the large intestine of the same dog, showing the degree of inflammation in that portion of the alimentary canal.

It may here be observed that half an ounce of the saturated solution of arsenious acid produced in several instances no symptom of ailment even when injected into the veins.

Various experiments were made by Dr. Roupell with corrosive sublimate in solution. Half an ounce of the liquor hydrargyri oxymuriatis P. L. which contains a quarter of a grain of corrosive sublimate, injected as before into the veins, gave rise to no apparent inconvenience.

The injection of a whole ounce containing half a grain of sublimate produced marked discomfort to the animal, followed by severe vomiting and dysentery. But although the symptoms of irritation in the abdomen were urgent, and the dejections proved the highly inflammatory state of the bowels, yet death did not ensue from that quantity.

Dr. Roupell's next experiment was made with tartarized antimony, with the vinum antimonii tartarizati of the London Pharmacopœia. An ounce of this liquid was thrown into the saphæna vein of an active terrier dog. The vinum antimonii tartarizati contains two grains of tartarized antimony, and rather more than a drachm of rectified spirit in the ounce. The immediate effect of this injection was to produce intoxication. The animal was able to move about; but his legs failed him, he seemed giddy, and his gait was staggering. No other effect was apparent for half an hour, and as the dog then seemed but little affected, it was left, under the supposition that the spirit had prevented the action of the tartar emetic. But when the animal was visited some hours afterwards it was found dead and stiff.

Judging from the appearance of the place where it had been confined and from the state of the jaws it was evident that vomiting had taken place. Examination was made next day. There was no morbid appearance in the brain; no morbid appearance indeed was detected anywhere except in the stomach, and that afforded a striking contrast to the other parts of the intestinal canal. These were of their natural white colour, while the stomach was of a vivid red, showing a high state of vascularity, great local determination, and intense inflammatory congestion. As the inflamed part presented no distinguishing peculiarity, it was not considered worth while to employ an artist to copy it. The stomach was inflamed throughout, and the inflammation extended from the cardia to the pylorus, not uniform however in degree, for the redness was greater at the larger end. This effect is so constant when all parts of the stomach have been equally exposed to the action of irritants as to induce the belief that there must be some difference in the vascularity of the two portions. The author had originally supposed that this inequality in the action of irritants was probably owing to the greater rapidity with which all matters passed over this part of the digestive tube, but he was inclined to believe from the diminished redness in this case that some other cause, and that probably a lower degree of vascularity, exists in this situation.

Although it may be presumed that the alcohol contained in the antimonial wine had delayed the effect of the tartarized antimony, yet it does not appear at all to have mitigated it, as it could hardly have been supposed *à priori* that death would have taken place in a dog from two grains of tartar emetic.

Dr. Roupell states that he made many other experiments which did not furnish results sufficiently precise to merit enumeration, or to which the effect produced could be distinctly referred to one cause. Thus when a solution of metallic salts have been injected into the veins, as the tinctura ferri muriatis, or the liquor plumbi acetatis, death has been quickly occasioned and the mucous membranes of the abdomen have presented a marked red appearance; but he has been unable to satisfy himself how far such changes were due to the presence of the metallic oxide, or how far they were the simple consequence of the acid. Either of these substances would produce one effect, the immediate coagulation of the blood, or would predispose to that condition. Not indeed that the mere coagulation of the blood in the veins will occasion any alteration whatever in the mucous membranes, none at least has been observed from the injection of the substance which has the greatest power in coagulating albumen, "kreosote," into the saphæna vein. No altera-

tion at all, no symptom whatever has followed the injection of half a drachm of kreosote mixed with water. When double that quantity has been injected pure, death has immediately ensued, apparently occasioned by the obstruction to the pulmonary circulation, the lungs having been found black and gorged with blood, which seemed composed of minute granules mixed with a fluid of inky blackness.

In the present state of animal chemistry, and in reasoning from so small a number of experiments, any explanation of the phænomena here detailed must be held to be conjectural. But such is in fact one great object of the Meeting. It would appear in the first place that arsenic injected into the veins exerts an influence primarily on the small intestines, that there at least its effect as an irritant begins; and as far as these experiments go it would seem that the upper part of the duodenum was the first to exhibit traces of its action. In the first experiment the large intestines were absolutely free from any organic change, and the stomach but slightly participated. When a longer period had elapsed, other portions of the gastro-intestinal mucous membrane have become inflamed, and other and remote parts have been implicated. Such a series of effects is seen in cases of poisoning by arsenic. In these the epigastrium is first referred to as the seat of derangement; then the whole alimentary canal; next the skin is the seat of an efflorescence, or rash, the urinary organs often participating at the same time. After a longer or shorter interval a crop of pustules will appear; later yet the nervous system is affected; paralysis comes on. Whether the bones would eventually become affected, as is the case with the cattle in the neighbourhood of manufactories where arsenical exhalations are generated, can only be inferred. In animals thus situated the joints become inflamed, anchyloses take place, and the bones enlarge and eventually become carious. The more minute series of vessels through which the fluids may be required to pass previous to entering into these various structures, as well as a diminished susceptibility in them, and the necessity consequently for a repetition of the stimuli, may perhaps offer some explanation of these progressive affections. That the vascularity excited by arsenic taken into the stomach will doubtless be allowed to result from some chemical effect. Inflammation would not be set up by the application of a merely inert powder to a mucous surface from simple contact. The cravings of hunger of certain Indians are appeased by devouring clay without exciting inflammation. Nor can the angular shape of the minute crystals be, as was formerly conjectured, the cause of the excitement of inflammation, for we know that large quan-

tities of pounded glass may be swallowed with impunity. That the inflammation produced by arsenic when circulated in the veins is also a chemical effect will no more be doubted than the other, as the author conjectures; for we know that many substances if soluble in the blood may be injected even in large quantities into the veins. Of this we have instances in the experiments of Orfila with the resin of jalap, and other substances. The operations then we may infer to be chemical, and inflammation to be excited in the part peculiarly susceptible of the action of the poison either because the chemical changes may take place in the part, or because the part itself may by idiosyncracy be disposed to resent that particular stimulus. Whether or not the system be chiefly on its guard against the introduction of such substances as tend to increase the disposition of the blood to coagulate may be matter of future consideration. Certain it is that metallic oxides and alcohol appear especially to excite the inflammatory condition. Equally certain it is that the elimination of the albuminous principles in the various forms of gelatine, albumen, and fibrine must be the means of the growth of natural tissues and the cause of the formation of many new structures. Albumen, it is well known, is precipitated by metallic salts, decomposition taking place, and a compound resulting, a combination of the oxide with the albumen. This compound, it is true, is again soluble in a liquid containing albumen in excess; a provision by which the formation of solid masses, which we saw in the blood of the dog poisoned by kreosote, would be prevented, and the fatal result which might otherwise ensue be obviated. But whether these chemical changes do actually occur, or whether any power of the system to prevent such an occurrence be called into play, an increased tendency to coagulation must be presumed to exist; the circulating fluids must have become more stimulating; and the disposition to a change of structure will have increased. Whether this may be the simple explanation of such albuminous deposits as are seen in the kidneys and inner coats of the arteries, especially in drunkards or in those addicted to spirit-drinking, is here advanced as appropriate matter for discussion. How far the agency of galvanism in the case of the oxides may operate in promoting coagulation may perhaps be more readily conjectured than shown. But when the coagulation of albumen is under consideration, it becomes impossible to avoid calling to mind the extraordinary facility with which that consolidation is produced by the electric fluid. And it will be enough to observe, that in all chemical changes, both of union and decomposition, this agent is in operation. What the

changes are which take place it would certainly be important to ascertain, that such is the fact, however an example or two may be cited to show. When mercury is extracted from the skin, deposited from the urine, or found in the bones of those who have been under a mercurial course, it is met with in the pure metallic form, although exhibited in that of a chloride; and iodine taken pure, and having passed through the system, is discovered in the form of iodide or hydriodate.

Speculations, it was observed, of this sort might be almost interminable. Bounds must therefore be set to such conjectures. Still the state of the blood in cases of poisoning is peculiar both in cases where the nervous energy has been highly excited, or when on the contrary it has been suddenly and greatly exhausted; where inflammation has been set up locally, or where a general inflammatory diathesis alone has been provoked. True it is indeed, and no less curious than true, that blood in the inflammatory state is less disposed to coagulate, or rather that coagulation takes place more slowly in blood drawn during inflammation than that taken from the vessels in its natural state. The author expressed the submission with which he laid these remarks before the Meeting, which, he observed, related only to those substances unquestionably taken into the circulation, and expressed his determination to dedicate his time and thoughts to the further elucidation of the ideas scattered through them, which he hoped to advance, and render more perfect before the next meeting of the Association.

# *Report on the Motions and Sounds of the Heart. By the* DUBLIN SUB-COMMITTEE *of the Medical Section.*

THE Committee having met together several times, and having considered different opinions hitherto advanced on the subject of the 'Motions and Sounds of the Heart,' proceeded to institute a series of experiments. The subjects chosen for experiment were calves, in which animals the heart is sufficiently large to admit of the motions and sounds being accurately observed; and their early age is favourable to a prolongation of the experiment, as it has been ascertained that the vitality of the different organs is more enduring, and less influenced by injuries to the individual, in animals at a very early age than in those of maturer growth.

The animals were prepared for experiment in the following manner: a tube, connected with a pair of bellows, was introduced into the trachea, and secured there, and the sensibility of the creature having been destroyed by a blow on the head, artificial respiration was established, by means of which the heart was enabled to continue its pulsations for a period varying in different subjects from one hour to two. The Committee had been unable to procure some of the Woorara poison, which has been used in similar experiments in London; and they found that the employment of prussic acid, in a quantity sufficient to suspend the sensibility of the animal, destroyed, in a few minutes, the power of motion in the heart.

## § 1. *Experiments on the Motions of the Heart.*

EXP. 1. A calf, two days old, having been secured on its back, and prepared as above described, the sternum, and a portion of the ribs on both sides were removed, when the heart was seen beating strongly, at the rate of 80 beats in the minute. While still inclosed in the pericardium, the heart was observed to have a slight libratory motion on its longitudinal axis, which motion, it may here be remarked, may assist in explaining the phænomenon of '*frottement*,' observed in disease. On cutting open the pericardium and turning it aside, both the auricular appendices were seen to project with a rapid motion upwards, or towards the place of the sternum, and immediately afterwards to recede. When coming upwards, they were swollen and soft to

the touch; when receding, they became hard to the touch, were diminished in size, and flattened. Immediately after the recession of the auricular appendices, the ventricles with a rapid motion assumed a somewhat globular form in their middle part, which projected upwards, and their apex at the same time was considerably elevated. During their continuance in this state the ventricles were hard to the touch, and if grasped with the hand at the commencement of the movement, they communicated a shock, or impulse, and separated the fingers. When the ventricles had remained for a short time in the state just described, they suddenly sank downwards, or towards the spine, and became elongated, broad and flat, and soft to the touch.

This succession of motions having been observed for some time, a small glass tube was introduced through a puncture into the left auricular appendix, and the blood was seen to rise in the tube during the recession of the appendix, and to subside during its upward movement. A similar tube was introduced through a puncture into the right ventricle, and a jet of dark-coloured blood was thrown forth during the globular and hardened state of the ventricles, and subsided when they became flat and soft. A puncture was made in the pulmonary artery, close to the right ventricle, and through it a stream of blood issued, synchronously with the jet from the ventricle. A tube having been introduced, through a puncture, into the left ventricle, and one of the mesenteric arteries having been exposed and opened, the jet from the ventricle was observed to precede the jet from the artery by an interval easily appreciable. The femoral artery was opened, and a similar observation was made as to the interval between the jet from the left ventricle and the jet from that artery. Previously to opening the chest of the animal the Committee had satisfied themselves that the beat of the heart, felt through the sternum or cartilages of the ribs, preceded the pulse, felt in arteries at different distances from the heart, by intervals of time which seemed proportioned to those distances; and they were also satisfied that the jets of blood from the femoral and mesenteric arteries were synchronous with the pulses felt in those arteries.

Exp. 2. A calf having been placed on its right side, a portion of the ribs on the left side was removed, the sternum, and a part of the cartilages on that side being left in their natural position, and the pericardium was opened. It was now seen that, when the ventricles assumed their hardened state, their apex and a considerable portion of their anterior surface were closely applied to the sternum; and when the hand was interposed between the latter and the surface of the ventricles, a strong com-

pression was exercised on the fingers during each movement of the ventricles towards the front of the chest. [By the term 'anterior surface' of the ventricles, is meant the surface corresponding to the one called 'anterior' in the human heart.] When the ventricles were in their softened state their anterior surface was sometimes in contact with the sternum, and sometimes a little removed from it; and, from the contemplation of this and the preceding experiment, the Committee were satisfied that the situation of the heart in the thorax is affected by the position of the body; which observation has been made also by others. For instance, in the recumbent state on the back, the heart recedes somewhat from the sternum: if the individual lie upon the face, the anterior surface of the ventricles is in constant apposition with the front of the chest, the pericardium, of course, being interposed. The yielding texture of the lungs, and the mode of attachment of the pericardium and great vessels, are such as to allow the gravitation of the heart to influence its position in different postures of the body.

These experiments were repeated on different animals, and the observations recorded above were confirmed.

Exp. 3. A rabbit was stunned, and its heart was immediately taken out of the body, and placed on the hand, with the anterior surface of the ventricles upwards. The ventricles continued to beat for some time, and assumed alternately the forms which have been described in the first experiment. During the continuance of the globular form, the body of the ventricles was protruded upwards, and their apex was elevated considerably from the hand; and while in this state it was ascertained, by measurement with a pair of compasses, that the length and the breadth of the ventricles were diminished. On the collapse, or softened state of the ventricles taking place, they became longer and flatter, and their apex sank towards the hand. The heart was now placed with the posterior surface of the ventricles upwards, and a globular swelling in their middle part was observed to alternate with a flattened form, in this aspect also; but the apex was not elevated as in the preceding part of the experiment.

Exp. 4. The sternum of a frog having been removed, the following appearances were observed. The ventricle, having become swollen, soft, and red-coloured, gradually sank and diminished in size, and became pale, and hard to the touch; alternating in these appearances with similar appearances in the auricle. It was manifest from the colour of both ventricle and auricle in their swollen state, that they were then full of blood, and from their softness, that they were in their diastole. When

they became pale and diminished in size they were in their systole. During the diastole of the ventricle its anterior surface was prominent, and approached the sternum, while its apex drooped towards the spine. In its systole, the anterior surface receded from the sternum, and its apex was slightly turned upwards. The finger being applied to the ventricle during its systole, a slight shock or impulse was felt.

In this experiment, the relations between the sternum and ventricle, during the diastole and systole of the latter, are nearly the reverse of those observed in the heart of quadrupeds in the preceding experiments. In them the ventricles approach the sternum during the hardened state, or systole, and recede from it in the softened state, or diastole. This difference depends upon the dissimilarity of the heart in the warm-blooded and in the cold-blooded animals, and will be adverted to again.

### § 2. *Experiments on the Sounds of the Heart.*

Exp. 5. A stethoscope was applied on the sternum over the heart, in a calf in which artificial respiration had been established, and both sounds of the heart were distinctly heard: the first prolonged and dull, the second abrupt and clear. The sternum and ribs were removed, so that the heart moved free from contact with any part of the thorax; and a flexible ear-tube having been placed on the pericardium, over the ventricles, both sounds were distinctly heard. [In the experiments on the sounds of the heart, with the sternum removed, the flexible ear-tube was found to be of much service, in preventing the transmission of the shock or impulse which was felt when the common stethoscope was used, and which embarrassed the observations.] The ear was now applied near to but not touching the heart, and both sounds were distinguishable, but feeble. A small piece of board was placed over the surface of the ventricles, and kept in contact with the pericardium, and by a stethoscope applied upon the board, both sounds were heard as distinctly, and very nearly as strongly, as when heard through the sternum. The ear-tube was placed on the ventricles, near their apex, and in this position the first sound was very distinctly heard; the second sound indistinctly. When the ear-tube was placed over the origins of the large arteries, both sounds were heard distinctly, the second particularly so. The pericardium was distended with tepid water, and in that state both sounds were heard by the ear-tube applied to its surface, but not so clearly as before the injection of water.

Exp. 6. In a calf, the sternum and ribs having been removed

as in the last experiment, and the pericardium having been cut away, both sounds were heard, by the ear-tube applied to different parts of the ventricles, in the same manner as in the last experiment. The great arteries were compressed close to the heart; and the character of the second sound was altered; and at times it seemed to some of the Committee that the second sound was lost, the first sound remaining. A fine curved needle was passed into the aorta, and another into the pulmonary artery, beneath the line of attachment of one of the semilunar valves in each vessel; and the needles were passed upwards, about half an inch, and out again through the respective vessels, so as to confine a valve in each, between the needle and the side of the artery. Upon applying the ear-tube over the origins of the arteries, it was found that the second sound had ceased, and that a sound resembling the first, and coinciding with the systole of the ventricles, was still audible. Some of the members of the Committee thought that the sound just mentioned was prolonged beyond the usual duration of the 'first,' or 'dull' sound, which had been heard before the introduction of the needles; and, towards the termination of the experiment, it was observed by some of those present, that there seemed to be a repetition of the sound called 'first', or to be two prolonged sounds, similar to each other, and which might be characterized as 'rushing' sounds. When the heart was removed from the body, and the semilunar valves examined, it was found that one valve in each artery had been confined against the side of the vessel, so as completely to prevent its descent. [It may be remarked that this operation can be very easily performed, and almost with certainty of success.]

Exp. 7. The foregoing experiment was repeated on another calf, and with the same result,—the cessation of the second sound. During the experiment the second sound, somewhat modified, was heard to recur; and, upon examination, it was found that the needle which had been passed into the aorta had slipped out. On its being replaced, the second sound again ceased. On taking out this heart also, the valves were found to have been confined, as in the last experiment.

Exp. 8. A calf having been stunned, the heart was taken out quickly, and placed on the table. The ear-tube was applied to the surface of the ventricles while still beating, and at each systole a sound was heard resembling that called the 'first' sound: no second sound was audible. When the heart had ceased to beat, the ventricles were filled with water, and the heart being held upright, the ear-tube applied to the ventricles; and these suddenly compressed with the hand, a sound resem-

bling the 'first' sound was heard. Also when the grasp of the hand was suddenly relaxed, a sound was heard of the same character as the one preceding. The ear-tube having been applied to the ventricles in the dead and empty heart, and their internal surfaces being rubbed against each other, a sound was heard much resembling the 'first' sound: and the finger having been introduced into the left ventricle, and being gently rubbed against the internal surface, a sound, also resembling the 'first' sound, was produced.

A glass tube allowed to drop from a small height on the semilunar valves of the aorta, caused a sound having the character of the second sound; and when the tube was passed between the valves, and gently rubbed against their edges, a sound resembling the '*bruit de rape*' was heard.

The foregoing experiments were frequently repeated, and the observations were confirmed.

§ 3. *Conclusions respecting the Motions of the Heart.*

From the preceding experiments on the motions of the heart the following conclusions may be drawn. 1. In the heart of warm-blooded animals the systole of the ventricles follows, immediately, the systole of the auricular appendices. 2. During the systole of the ventricles the auricles are distended by blood passing from the venous trunks. 3. The ventricles, when their systole has ended, become relaxed and flaccid, and the blood passes rapidly, but with little force, from the auricles intotheir cavities. 4. The auricles are never emptied of blood, and contract but little on their contents; an active contraction being observable only in their appendices. 5. If the interval between two successive beats of the heart be regarded as divided into four equal parts, two of those parts may be allotted to the duration of the ventricular systole, rather less than one part to the interval between the end of the ventricular systole and the commencement of the diastole of the appendices, during which interval little motion is observable in the auricles, and the remaining portion may be allotted to the diastole and systole of the auricular appendices. 6. The ventricles, in their systole, approach the front of the thorax, and by their contact and pressure against it produce the impulse, or 'beat' of the heart. 7. The beat of the heart and pulse in the arteries are synchronous only when the pulse is felt in arteries close to the heart: in those at a distance the pulses are later than the beat of the heart by intervals of time proportioned to the distances.

In the heart of the frog, which was examined in the fourth experiment, the ventricle swelled and approached the sternum

in the diastole, and receded from it in the systole. This difference between the movements of the heart in that animal and in the others which were submitted to experiment, may be explained by considering that in the heart of the latter the swelling of ventricles during systole is produced by the thickening of their muscular fibres, which are then in a state of contraction, and of which the mass bears a large proportion to the size of the internal cavities: while, in the heart of the frog, the sides of the ventricle are thin, and its cavity is large; and the thickening of its sides produced by the contraction of their fibres, does not counterbalance the diminution of the volume of the ventricle attendant on the expulsion of its blood.

### § 4. *Conclusions respecting the Sounds of the Heart.*

From the experiments on the sounds of the heart it appears to follow: 1. That the sounds are not produced by contact of the ventricles with the sternum or ribs, but are caused by motions within the heart and its vessels. 2. That the sternum and front of the thorax, by their contact with the ventricles, increase the audibleness of the sounds. 3. That the first sound is connected with the ventricular systole, and coincides with it in duration. 4. That it is not produced by the friction of the internal surfaces of the ventricles against each other, as such friction cannot exist until the blood has been expelled from the ventricles, whereas the first sound commences with the beginning of the ventricular systole. 5. That the cause of the first sound is one which begins and ends with the ventricular systole, and is in constant operation during the continuance of that systole. 6. That the first sound does not depend upon the closing of the auriculo-ventricular valves at the commencement of the ventricular systole, as that movement of the valves is of an instantaneous character, and is much shorter in duration than the systole. 7. That it is produced by the rapid passage of the blood over the irregular internal surfaces of the ventricles, on its way to the mouths of the great vessels. 8. That the '*bruit musculaire*' may contribute to the production of the first sound. 9. That the second sound coincides with the termination of the ventricular systole, and requires for its production the integrity of the semilunar valves of the aorta and pulmonary artery; That it is caused by the sudden check given by the action of those valves to the motion of the columns of blood driven towards the heart after each ventricular systole, by the elasticity of the arterial trunks.

The Committee wish to express their opinion, that although

much light has been thrown on the motions and sounds of the heart by recent investigations, here and elsewhere, the nature of the inquiry is such as to render it difficult in many instances to arrive at satisfactory conclusions. They also think that the subject is one which, from its importance, whether in a practical view, or as an object of philosophical inquiry, is deserving of further investigation.

*Signed*,

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Bruce Joy, M.D., Fellow of the College of Physicians.
John Nolan, M.D., Demonstrator of Anatomy in the University of Dublin.
Erory Kennedy, M.D., Master of the Lying-in Hospital.
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## *Report on the Registration of Deaths. By the* EDINBURGH SUB-COMMITTEE.

DR. ALISON reported from the Edinburgh Sub-committee appointed in 1834 to consider the subject of *registration of deaths*, with a view to a legislative measure as to registration, (see Proceedings of the Edinburgh Meeting, p. 39,) that they had drawn up a paper of suggestions on this subject, which they had proposed to the London Sub-committee as proper to be submitted to the consideration of those Members of Parliament who might interest themselves particularly in the Registration Bills for England and Scotland about to be introduced;—that the London Committee had expressed some doubt as to the application of these suggestions to the case of the English Bill, but after some explanations had acquiesced in the propriety of their being transmitted *simpliciter* to the gentleman who had given notice of his intention to bring into Parliament the Registration Bill for Scotland.

The Section of Anatomy and Medicine having heard their paper read, directed that it should be communicated to the Statistical Section, with a request that they would give their attention to the subject; and if they concurred in the opinion of its importance, that they would take such steps as they might think expedient to bring those suggestions (with such modifications as they might judge proper,) under the view of those Members of Parliament who might be likely to take a share in the preparation of legislative measures on this subject.

### *Suggestions by the Edinburgh Sub-committee.*

There are many questions regarding the external causes of diseases, and the means of preventing them, susceptible of more direct application to the good of the public, than most discussions on their nature, on which it is hardly possible for individuals, within the sphere of their own experience, to acquire satisfactory information; and which have on that account been hitherto very imperfectly investigated. Every individual case of disease, or of death from disease, is probably determined by several external causes, the respective influence of which is very easily misapprehended; and it is only by multiplying *very*

*greatly* the numbers of observations, that such sources of fallacy, attending any single cases, can be avoided; and general laws, touching the influence of such causes, be satisfactorily deduced.

Thus it is in general only by observing that a particular disease affects a much greater number of those persons who are known to have been exposed, to the agency of a particular external circumstance, than of those who are not known to have been exposed, that we learn that such circumstance has power to cause that disease. It is very seldom, particularly in civil life, that we can have observations, as to the influence of such a cause, of the nature of the *experimentum crucis*; *i. e.* when all other circumstances in the condition of the persons observed are exactly alike, excepting only the presence of that cause in one set of cases, and its absence in another. But it may always be presumed that out of *a very great number of cases* in which one condition has been uniformly present, all other conditions must have been applied very variously; and therefore, by very greatly multiplying the number of observations, where one alleged cause has been applied, we may ultimately get rid of the source of fallacy, resulting from the varying conditions of each single observation, and from a fair estimate of the efficacy of the particular cause in question.

Thus also the experience of an individual, even if carefully preserved, goes but little way in ascertaining the effects of seasons, of localities, of occupations, or modes of life on the mortality of any given disease, because in every individual case which has been under his observation, the influence of any one of these causes must always have been combined with that of others, which may have determined the result; but if the experience of a *very great number* of individuals on the mortality from that disease, under the influence of one of these causes, is exhibited at once, it may fairly be presumed that all accidentally concurrent causes must have acted so variously on so great a multitude, that the irregularities thence arising must have destroyed one another, and that the influence of permanent and general laws only will be perceptible in the result. Many attempts have accordingly been made by medical men to acquire more certain information, as to the comparative efficacy of different causes of disease and mortality, than the experience of individuals can supply, by reference to registers of deaths, kept in different situations, and extending to large numbers of persons and to long periods of time. But these attempts have been in a great measure frustrated, or at least their results rendered much less certain and important than they would otherwise have been in this country, by the imperfect and irregular

manner in which such registers are kept; and it would therefore be matter of very serious regret for the interests of humanity, as well as of medical science, if any legislative measure in regard to registration should become a law, without care being taken to secure that the registers of mortality shall be *kept on a uniform plan in all parts of the King's dominions*, and in such a manner as to afford all the information relative to the *causes of mortality* which can reasonably be expected from such records. Some of the provisions in the Bills for registration in England and Scotland which were last year brought into Parliament, appear to be well calculated for promoting the purposes here stated: and in particular the provisions that books for registration be kept by persons of some intelligence in every parish and every town throughout the country, and that *no interment shall be permitted to take place in any burial-ground without a certificate of the registration of the death being produced*, appear quite indispensable to the proper regulation of this matter; but the forms furnished in the two Bills for keeping the registers of death *are materially different*, and in several respects *both forms appear essentially defective*, and would certainly fail of affording all the information which it is desirable, and certainly practicable, for such a register to give.

The second column of the Schedule C. of the Scotch Bill, intended to record the designation and place of abode of the deceased person, for greater precision and minuteness should certainly be divided into two, and the *rank or employment* of the person, past or present, or *that of his father, or of the head of the family* in which he lived, be stated in one; and the *exact residence*, *i. e.* not merely the town, village, or parish, but the street and number, or the division of a parish, in the other; and another column should be added here, for recording the *exact age* of every deceased person.

In like manner in Schedule B. of the English Bill there should be a column to indicate the *exact residence*; and the column to mark the rank or profession of the deceased should state also "*or that of the head of the family.*" It is quite essential, for the purposes that have been stated, to have such a record of the mortality, not only of each *town* or *parish*, but in *every occupation or line of life*, at *every age* from infancy upwards, and in *every description of locality* (*high or low*, *damp or dry*, *town or country*, *district* of a parish, &c.) as shall enable any inquirer, by examination of registers, to obtain and exhibit information on these points, for any given time, *in the form of tables*, and it is obvious that for that purpose *the age*, the *exact residence*, and some indication of the *occupation* or *rank* in *life* of each indi-

vidual must be recorded; and these are points on which accurate information may in almost in every case be easily obtained without the instrumentality of any other agents than those already contemplated in both Bills.

Again, it is of the utmost importance to have as accurate information as can be obtained as to the *causes of death*; and although there may, for a long time to come, be a deficiency of precision in the statements of that kind which may be procured, yet it will certainly be right to have a column (as directed in the Scotch Bill), and even to divide this into two compartments, for this purpose. On one important point information may always be had, viz. as to whether the fatal disease was acute or chronic, by an answer being required to the query, whether the deceased was ill, and disabled for his ordinary occupation, for less or more than six weeks (or 40 days) before death. What occurs to the Sub-committee as the best expedient for obtaining further information, is, that the Bill should contain a clause directing the names of diseases to be entered according to regulations to be subsequently issued by the Secretary of State for the Home Department. The substance of these regulations the Sub-committee think should be as follows:

A list of diseases should be furnished to each keeper of registers, and he should be directed to inquire of each person registering a death, whether he can state, on the authority of a medical practitioner, that any one of the diseases in that list was the cause of death; if so, that name is to be entered under the head of acute or chronic disease according to the rule already stated. If no medical authority can be given for the name of the disease, the keeper of the register should be directed to inquire whether any prevalent epidemic was the cause of death; and if not, whether the *part of the body* chiefly affected in the disease of which the person died was *the head*, as in apoplexy, palsy, convulsion; or *the chest*, as in inflammation, or consumption, of the lungs, asthma, dropsy with difficult breathing, &c.; or the *lower bowels*, as in inflamed bowels, dropsy of the belly, flux, jaundice; or the *external parts*, as in diseases of the joints, limbs, or surface of the body. The cause of death is then to be entered as disease of the *head*, *chest*, *lower bowels*, *limbs*, or *surface of the body*; and under the head of acute or chronic disease according to the rule above stated. By means of such regulations, duly enforced throughout the kingdom, the Sub-committee think that such information may be recorded as, if thrown by future inquiries into the form of Tables, may very greatly elucidate the causes, and the means of prevention, at least of the most important diseases; and this by means of the same agents

as are required to be employed by the Bills now in contemplation, and without imposing any expense on any party beyond what the other provisions of the Bills have already done.

Thus the Schedule to be inserted in the Bill for directing the mode of registering deaths, so far as the information that is desirable for medical inquirers is concerned, would stand thus:

| Date. | Name | Age | Exact Residence. | Employment, or that of the head of the Family. | Disease or cause of Death. | | |
|---|---|---|---|---|---|---|---|
| | | | | | Acute. | Chronic. | |
| | | | | | | | |

As it may hereafter appear practicable and expedient to require for certain times, and in certain places, some further and more precise information, it seems highly desirable that two or more blank columns should be directed to be left in every book kept for the purpose of registration, and that the Executive Government should reserve to itself the power of hereafter directing that those columns shall be filled up in such manner as may be thought proper, without the expense and loss of time requisite for a new Act of Parliament.

# NOTICES

# COMMUNICATIONS

TO THE

# BRITISH ASSOCIATION

# ADVANCEMENT OF SCIENCE;

AT DUBLIN IN AUGUST 1835.

LONDON:

JOHN MURRAY, ALBEMARLE STREET.

1836.

# ADVERTISEMENT.

The Editors of the following Notices consider themselves responsible only for the fidelity with which the views of the Authors are abstracted.

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# ABSTRACTS OF COMMUNICATIONS

TO

# THE BRITISH ASSOCIATION

FOR THE ADVANCEMENT OF SCIENCE,

AT

## THE DUBLIN MEETING, AUGUST 1835.

[*From the London and Edinburgh Philosophical Magazine and Journal of Science.*]

THE contributions to science received at the annual meetings of the British Association are of two classes,—the one consisting of reports and researches executed under its immediate impulse and direction, the other of miscellaneous communications, the authors of which choose this method of bringing new facts or theories into notice, and of submitting them to public discussion.

Without undervaluing in any degree the latter class of contributions, the Association deems it advisable to deal with them in such a manner as to avoid any interference with the Transactions of other Institutions: with this view it has discouraged the production, at its meetings, of papers in a state for publication in such Transactions; and whilst it prints at full length those reports and researches which are directly its own, it has refrained from publishing the miscellaneous communications in any other form than that of notices and abstracts.

At the last meeting it was determined to draw the line of distinction still more completely, and at the same time to afford a speedier opportunity of publishing views brought forward for the sake of early notice and discussion, by transferring the abstracts of all the miscellaneous papers communicated to the Meeting from the annual Report of the Association to the periodical journals of science.

As in regard to the number and value of scientific contributions, so in other respects the meeting at Dublin fulfilled

all the expectations which had been entertained of its success: even before it assembled there its members had received such unusual proofs of the esteem in which the Association was held as could not but add to the spirit and animation of the meeting. The tribute to science paid by an eminent merchant of Liverpool (Sir John Tobin) in devoting one of the finest steam-boats in that port to the service of its members, and accompanying them in three voyages as their host; the kindred spirit evinced by the Directors of the Dublin and Kingstown rail-road, who provided gratuitous conveyance from the coast to the capital; the splendid entertainments given in the Zoological and Botanic Gardens; the hospitalities of the Royal Colleges of Physicians and Surgeons, and of that illustrious academical body on which rested the chief charge and credit of receiving the Association; the participation in these festivities of the Representative of the Sovereign, and the happy manner in which he seized the occasion of conferring a public mark of distinction on the highly-gifted mathematician and astronomer who held office as one of the Secretaries of the Meeting;—in addition to these open testimonies of respect for scientific pursuits, the silent undercurrent of refined and invisible hospitality by which the guests of Ireland found their expenses contracted and their cheer enhanced,—all these were indeed but collateral circumstances attending that meeting, and managed in such a manner as to interfere with none of its scientific labours; but they were not ineffective in kindling a warmth of feeling by which the powers of the mind are capable of being invigorated even in the pursuit of abstract truth. The moral calm, too, which the meeting seemed to communicate,—the suspension of every feeling but that of a common interest in promoting the knowledge of nature,—this, in like manner, was but an incidental circumstance, yet it raised thoughts of the usefulness as well as the dignity of those studies which possess a charm not only to elevate the individual but to bind the species together.

Reflections of this kind, which crowd upon the mind on such occasions, and which the meeting at Dublin excited in a peculiar degree, contribute their share to that general effect of which Professor Hamilton gave so eloquent a description in his preliminary address, whilst asserting the power of social sympathy over the most private moments of exertion in the secret retirements of science. "We meet, we speak, we feel," said the Professor, "*together now*, that we may hereafter the better think and act and feel *alone*. The excitement with which the air is filled will not pass at once away; the influences that are now amongst us will not, we trust, be

transient, but abiding: these influences will be with us long; let us hope that they will never leave us: they will cheer, they will animate us still, when this brilliant week is over; they will go with us to our separate abodes, will attend us on our separate journeys; and whether the mathematician's study, or the astronomer's observatory, or the chemist's laboratory, or some rich distant meadow, unexplored as yet by botanists, or some untrodden mountain top, or any of the other haunts and homes and oracular places of science, be our allotted place of labour till we meet together again, I am persuaded that those influences will operate upon us all, that we shall all remember this our present meeting, and look forward with joyful expectation to our next reassembling, and by the recollection and the hope be stimulated and supported."

Highly, however, and justly as we prize the social and sympathetic ardour of mind which these meetings spontaneously produce, we must not confine our views to this object in such a manner as to propose to dispense with more direct endeavours to effect the advancement of science. On this subject some remarks were offered by Mr. Harcourt, at the close of his statement of the Recommendations of the Committee and of the appropriation of certain sums to scientific purposes.

After adverting to some remarkable instances which had come to his knowledge of the actual effect of these meetings in awaking the dormant spirit of science, and enumerating among the indirect benefits that arise from them the means which they supply to persons whose merits have been obscured by accidental circumstances, of vindicating their own rightful claims, and of repelling that false and partial criticism by which genius had in former days been too often depressed, he proceeded to say, "After all, every important advantage which these meetings possess, and, above all, the maintenance in them of the true principles and character of philosophical investigation, will entirely depend on the continued presence and concurrence of the *master-spirits* of science; and it must be remembered that these are the persons whose attendance, from the value of their time, it is most difficult to secure. From the first commencement of the Association I have always held that there is but one motive strong enough to tear those persons from their retirements and to bind them to these annual meetings. If you here offer to them the direct and acknowledged means of advancing the science to which they are attached, if you assist the astronomer in effecting the reduction of the elements of his calculations, if you establish for the meteorologist a system of conjoint and extended observations from which the

laws of the atmosphere may be deduced,—with such objects before them, the greater mastery they may possess in science the more eager will be the interest which they take in your meetings, and the more probable it is that you will enjoy the advantage of their counsel, and the communication of their spirit, than which there is nothing more essential to give life and consistence to your proceedings."

This we are persuaded is the vital principle on which the permanence of the Association depends. Should it ever be lost sight of, should the resources of the institution come to be expended chiefly on subordinate objects, and its recommendations directed to little points, instead of the great questions which interest men of comprehensive views in the different departments of science, the consequence will be that the meetings will be left entirely to men of second-rate acquirements, and that they will speedily fall into contempt.

We have reason to hope that the next volume of the Transactions of the Association, which we are informed will soon appear, may bear evidence of a continued attention to this principle; in the mean time the answer contained in Mr. Hamilton's address to the objection of a writer in the Edinburgh Review against the exercise of the influence of the Association in obtaining from the Government a grant of money for the reduction of observations on the sun, moon, and planets, made at Greenwich by Bradley and his successors, sufficiently shows how judiciously it has commenced its operations. The astronomer royal of Ireland informs us that the particular undertaking thus objected to has afforded the most unmixed gratification to those cultivators of science who are interested in the progress of the highest department of astronomy, and he quotes the opinion of Bessel to the following effect: "To me, considering all these things together, it appears to be of the highest moment towards our future progress in the knowledge of the solar system, to reduce into catalogues, as conveniently as can be done, according to one common system of elements, the places of all the planets observed since 1750; than which labour I believe that no other now will be of greater use to astronomy."

We must refer to the Reports of the Association for further proofs, in discussions of tables of the tides and other important investigations, that there is no want of enlarged views in its Recommendations and in the expenditure of its now considerable funds. As long as this continues to be the case we have no doubt that, meet where it will, its meetings will attract a large proportion of those who are sincerely devoted to science for its own sake, and who have a just un-

derstanding of the spirit in which it is to be pursued and the methods by which it is to be advanced.

Nor does there seem to be any reason to fear that the want of a locality for such assemblages will be found to place an impediment in their way. At the late meeting there were deputies present from five of the chief commercial towns in England to invite the Association and to offer suitable accommodation in their respective towns. Bristol stood first on the list of those from which invitations had been received on former occasions; and its situation being also far removed from the districts which the Association has hitherto traversed, it was determined to hold the ensuing meeting in that city in August next. The highly interesting and important country which forms the South-west of England will be conveniently embraced by this meeting, and the zeal which public bodies no less than individuals have shown to facilitate and encourage the arrangements for it, concurs with the high reputation of the men of science connected with Bristol, to hold out the confident expectation of a successful result.

## NOTICES OF LECTURES DELIVERED AT THE EVENING MEETINGS OF THE ASSOCIATION.

Professor Powell gave a lecture on the phænomena of prismatic dispersion, in relation to the undulatory theory of light.

After giving a general view of the phænomena, and a particular description of the black lines in the spectrum whose position is taken as a measure of the refractive and dispersive powers of substances, Professor Powell proceeded to state the results of some recent labours undertaken by himself in order to ascertain whether the undulatory theory of light, which is admitted to explain almost every fact in optical science except dispersion, could be applied to explain that also. By reducing to calculation a formula suggested to the author by Professor Airy, as arising out of the researches of M. Cauchy, and expressing a relation between the refractive index of a ray and the length of the wave, a very close agreement was found between the numbers which result from the formula and those observed by Fraunhofer for ten different media, viz. four kinds of flint glass, three of crown glass, water, oil of turpentine, and solution of potash. Professor Powell is engaged in the arduous labour of testing the applicability of M. Cauchy's modification of the undulatory theory to the explanation of the phænomena of prismatic dispersion, by individual examples; and he states, that as far as the calculations have been executed, it appears that even the extreme case of that highly dispersive substance oil of cassia is comprehended with at least considerable accuracy by the theory. It appears, then, that

one of the greatest of the remaining objections to the reception of the undulatory theory is at least partially removed.

The Rev. W. Whewell stated the progress which had been made during the past year in Observations of the Tides, not only round the coasts of Great Britain and Ireland, but also under the direction of the Governments of Sweden, Denmark, Russia, Spain, France, Holland, and the United States. The dependence of the velocity of the tide wave on the depth of the ocean channels was pointed out as an instance of the collateral benefits arising from the advancement of any one branch of knowledge; for, in consequence of the perfection of this branch of hydraulical science, it might be possible for geologists to acquire some valuable information concerning parts of the ocean where no plummet ever sounded and no line was ever cast.

Mr. Babbage explained his views of a method of Natural Chronometry derivable from an examination of the annual layers of growth in wood. Considering these layers as liable to vary in thickness according to favourable or unfavourable seasons, and any series of them in one tree capable of being coordinated with a contemporaneous series in another, by means of these irregularities, it was shown to be possible to arrive at an accurate knowledge of the period of existence of trees in which life had become extinct, or which had been long enveloped in peat bogs, provided a sufficient number of trees of intermediate periods, which had been subject to the same irregularities of annual growth, could be examined. The bearing of the inquiry on historical records of seasons and on geological speculations was pointed out, and the process which it would be most convenient to pursue in the application of the method clearly indicated.

Professor Sedgwick presented a general review of the labours of the Geological Section during the week, illustrated by a section of the Cumbrian and Silurian systems of rocks.

Dr. Lardner delivered a lecture on Locomotive Engines, illustrated by drawings and working models.

Dr. Barry gave an account of his ascent of Mont Blanc in 1834, illustrated by drawings.

Mr. Babbage described a remarkable Phænomenon in the Sea on the coast of Cephalonia (details of which had been communicated to him by Lord Nugent), which appeared to indicate the existence of a large cavity below the bed of the sea, and communicating therewith.

Professor Wheatstone exhibited his Speaking Machine, and explained the principles of its construction, and the progress which had been made in the mechanical imitation of the human voice.

# NOTICES AND ABSTRACTS OF MISCELLANEOUS COMMUNICATIONS TO THE SECTIONS.

## MATHEMATICS AND PHYSICS.

Professor Hamilton gave a sketch of his new theory of logologues and other numbers of higher orders; (see Transactions of the Royal Irish Academy;) also a similar account of his new theory of varying orbits.

He likewise explained to the Section the method of investigation pursued by Mr. G. B. Jerrard, for accomplishing the solution of equations of the fifth or of higher degrees.

### *A short Account of some recent Investigations concerning the Laws of Reflexion and Refraction at the surface of Crystals. By Mr.* M'Cullagh.

To understand the nature of the general problem which a complete theory of double refraction requires to be solved, let it be supposed that a ray of light is reflected and refracted at the separating surface of an ordinary medium and a doubly refracting crystal, the light passing out of the former medium into the latter. This limited view of the subject is taken merely for the sake of clearness of conception; since we might suppose that both media are crystallized, without increasing the difficulty of the problem. The question, it is obvious, naturally divides itself into two distinct heads. The first relates to the laws of the *propagation* of light in the *interior* of either of the two media, before or after it has passed their separating surface; and this part of the subject has been fully treated, according to their different methods, by MM. Fresnel and Cauchy. The second division of the subject had been left completely untouched. It relates to the more complex consideration of what takes place at the separating surface of the media, the laws according to which the light is there divided between the reflected and refracted rays, including a determination of the attendant circumstances indicated by the wave theory, with regard to the vibrations in the reflected and refracted rays. In the case above mentioned, when the incident light is polarized, there are four things to be determined, namely, the *magnitude* and *direction* of the reflected vibration, with the *magnitudes* of the two refracted vibrations. The four conditions necessary for this determination are furnished by two new laws, which could not be easily stated without entering too much into detail. The results, applied to determine the polarizing angle of a crystal in different azimuths of the plane of reflection, agree very closely with the admirable experiments of Sir David Brewster on Iceland spar. In the course of these experiments it was observed that the polarizing angle remained the same when the crystal was turned half

round (through an angle of 180°), although the inclination of the refracted rays to the axis of the crystal was thereby greatly changed. This remarkable fact is a consequence of the theory. After some complicated substitutions in the primary equations, the value of the polarizing angle is found to contain only *even* powers of the sine or cosine of the azimuth of the plane of reflection, and therefore a change of 180° in the azimuth produces no change in the polarizing angle.

The two new laws above mentioned, on which the theory depends, occurred to the author in the beginning of last December; but, owing to an oversight in forming one of the equations, they were not fully verified until the beginning of June.

In this theory it is supposed that the vibrations of polarized light are parallel to the plane of polarization, according to the opinion of M. Cauchy. This is contrary to the views of Fresnel, whose theory of double refraction obliged him to adopt the hypothesis that the vibrations are perpendicular to the plane of polarization. It is further supposed, that the density of the vibrating æther is the same in both media; and this hypothesis of a constant density in different media, which was found necessary for the theory, seems to accord, better than the supposition of a varying density, with the phænomena of astronomical aberration.

If we conceive the three principal indices of refraction for the crystal to become equal, we shall obtain the solution of a very simple case of the general problem with which we have been occupied,—the case of an ordinary refracting medium, such as glass. This simple case, it is well known, was solved by Fresnel. The foregoing theory leads to a simple law, expressing all the particulars of the case, but differing with regard to the *magnitude* of the refracted vibration, from the formulæ of Fresnel. The law may be stated, by saying that *the refracted vibration is the resultant of the incident and reflected vibrations*; the first vibration being the diagonal of a parallelogram of which the other two vibrations are the sides, just as in the composition of forces. The plane of this parallelogram is the plane of polarization of the refracted ray. It is to be remembered, that the vibrations in each ray are perpendicular to the ray itself, and *parallel* to its plane of polarization.

This simple case has also been considered by M. Cauchy, in a short paper inserted in the *Bulletin Universel*, tom. xiv.; but it does not seem to have been observed by any one that his solution is erroneous. His formula for light polarized parallel to the plane of reflexion, is that which belongs to light polarized perpendicular to the plane of reflexion and *vice versâ*.

Mr. Whewell read his report on the Mathematical Theories of Electricity, Magnetism, and Heat.

[This report will be printed in the next volume of the Transactions of the Association.]

### *On certain points connected with the recent Discoveries relative to Radiant Heat. By Professor* POWELL.

In this communication the author expressed his particular satisfaction in finding that M. Melloni (in his second memoir) describes a repetition of the experiment originally made by him, and recorded in the Philosophical Transactions, 1825, with perfect success, by means of his extremely delicate apparatus. The confirmation is the more complete, as the experiment was made by M. Melloni with a different view.

It is thus now established beyond question, that luminous hot bodies are sending out two distinct sorts of heat, or two distinct heating agents, at the same time, differing in their properties and mode of operation.

Hence the whole series of results of M. Melloni must be interpreted with reference to this distinction, and possibly the consideration of it may remove some of the apparent anomalies.

Another question of importance which has occurred to the author is this,—Whether, in the polarization apparatus, supposing one glass or pile of mica heated, it will radiate the same quantity of heat to the other in the two rectangular positions. The question is purely a mathematical one, and has been in some degree considered, at the author's suggestion, by Mr. Murphy, of Cambridge. The integration has not been completed, but Mr. Murphy thinks it clear that there will be a difference.

### *On the Phænomena usually referred to the Radiation of Heat. By* HENRY HUDSON, *M.D., M.R.I.A., Dublin.*

For the purpose of repeating Leslie's experiments with variations of the temperatures of the surface of the mirror and of the thermometer, the author procured a parabolic zinc mirror with a hollow back, so that its surface could be heated or cooled by filling it with hot or cold liquids.

The following are the results obtained: 1st, Whatever be the temperature of the room, if the mirror and canister be at the same temperature also, there is no effect produced by either the metallic or the varnished side of the canister. 2nd, If the canister (alone) be above the temperature of the air, the varnished side produces a greater heating effect than the metallic side, in the proportion of about 12 : 1. 3rd, If the canister (alone) be *below* the temperature of the room, the varnished side produces a greater *cooling* effect than the metallic in the *same proportion* of about 12 : 1. 4th, If the mirror be heated considerably (say to 200° Fahr.), and the thermometer so arranged that both balls are *equally* warmed by the mirror (one of them being in the focus), a canister (at the same temperature as the room) produces a *cooling* effect on the focal ball, and the varnished side displays its superior efficiency. 5th, The mirror and thermometer being as in the last experiment, the canis-

ter was heated 10 or 12 degrees beyond the temperature of the room. The effects were now found to vary according to the distance of the canister from the mirror. At a short distance it acted as a cold body, and the varnished side most efficient; on increasing the distance, the effect diminished, and at a certain point altogether ceased; the thermometer marking zero, whether the varnished or metallic side was towards it; but on increasing the distance, the canister began to act as a warm body, and again the varnished side displayed its superiority. 6th, When the focal ball (merely) was *cooled* by the evaporation of water, or even of æther, neither side of the canister produced any change in the effect. 7th, When the focal ball was cooled 27° of Fahrenheit (by evaporation of æther), and the canister cooled 16° of Fahrenheit (being of course 11° warmer than the focal ball), the focal ball was now cooled more than previously, as if the canister were (comparatively) a cold body. The rapid evaporation of the æther makes these experiments troublesome. The author then pointed out that no theory of the *emission* of rays of heat could account for the phænomena, unless rays of cold were also admitted; and called attention to Professor Leslie's theory, as deserving further investigation, without, however, drawing any conclusion from the experiments, excepting that they could only be accounted for on *some* theory of undulations. He then suggested, as one cause of the different radiating powers of surfaces, their different capacities for heat. The two surfaces being at the *same* temperature and in the *same* medium (of a lower temperature), may be considered to have the *same* tendency to attain the common temperature of the medium, and may therefore be expected to give off the *same* portion of their excess of *temperature*, and consequently quantities of *heat* proportional to the *capacities* of the surfaces; taking the latter in the *physical* sense of having some definite thickness, which may be different in different substances.

Dr. Hudson then mentioned a few experiments made with Melloni's thermo-multiplier, respecting the question of the "direct free transmission of heat" through rock-salt, rock-crystal, and alum. Having removed the crystals from the opening in the screen, he moved the canister (containing hot water) entirely out of the axis of the thermoscope, so that the needle stood at zero. He then placed the crystals (successively) in the opening of the screen, and found the effects on the needle to be *instantaneous*, and also to follow the same *order* in the different crystals as to its amount, as when the canister was in the axis of the thermoscope, so as to make it *questionable* whether the effects in the latter case were not (*also*) wholly owing to the *conduction* of heat through the crystals. He alluded to *these* experiments merely as indicating a method of determining the point in question: as, if there be (contrary to Melloni's deductions) *no* direct transmission of simple heat, we may expect to find the same results produced by a given source of heat, whether in or out of the axis of the instrument, provided the canister and the crystal are equally distant, and their surfaces equally inclined to each

other in both cases. In the experiments with the mirror, he had used a differential thermometer containing æther instead of sulphuric acid, as being much more delicate in its indications of heat; and suggested its being made still more sensitive by the use of other liquids, having himself succeeded in making one containing *condensed* sulphurous acid gas.

*On the Prismatic Decomposition of Electrical Light. By Professor* WHEATSTONE.

The following is a brief notice of the principal results stated in this communication: 1. The spectrum of the electro-magnetic spark taken from mercury consists of seven definite rays only, separated by dark intervals from each other; these visible rays are two orange lines close together, a bright green line, two bluish green lines near each other, a very bright purple line, and, lastly, a violet line. The observations were made with a telescope furnished with a measuring apparatus; and to ensure the appearance of the spark invariably in the same place, an appropriate modification of the electro-magnet was employed. 2. The spark taken in the same manner from zinc, cadmium, tin, bismuth, and lead, in the melted state, gives similar results; but the number, position, and colours of the lines varies in each case; the appearances are so different, that, by this mode of examination, the metals may be readily distinguished from each other. A table accompanied the paper, showing the position and colour of the lines in the various metals used. The spectra of zinc and cadmium are characterized by the presence of a red line in each, which occurs in neither of the other metals. 3. When the spark of a voltaic pile is taken from the same metals still in the melted state, precisely the same appearances are presented. 4. The voltaic spark from mercury was taken successively, in the ordinary vacuum of the air-pump, in the Torricellian vacuum, in carbonic acid gas, &c., and the same results were obtained as when the experiment was performed in the air or in oxygen gas. The light, therefore, does not arise from the combustion of the metal. Professor Wheatstone also examined, by the prism, the light which accompanies the ordinary combustion of the metals in oxygen gas and by other means, and found the appearances totally dissimilar to the above. 5. Fraunhofer having found that the ordinary electric spark examined by a prism presented a spectrum crossed by numerous bright lines, Professor Wheatstone examined the phænomena in different metals, and found that these bright lines differ in number and position in every different metal employed. When the spark is taken between balls of dissimilar metals, the lines appertaining to both are simultaneously seen. 6. The peculiar phænomena observed in the voltaic spark taken between different metallic wires connected with a powerful battery were then described, and the paper concluded with a review of the various theories which have been advanced to

explain the origin of electric light. Professor Wheatstone infers from his researches, that electric light results from the volatilization and ignition (not combustion) of the ponderable matter of the conductor itself; a conclusion closely resembling that arrived at by Fusinieri from his experiments on the transport of ponderable matter in electric discharges.

*On the simultaneous Vibrations of a Cylindrical Tube and the Column of Air contained in it. By the Rev.* JAMES CHALLIS.

Mr. Challis, in his report on the Analytical Theory of Hydrodynamics, and elsewhere, has expressed the opinion that, to complete the theory of musical vibrations in a cylindrical tube, it is necessary to take into account the vibrations of the tube itself. In this communication he states some results which he has arrived at theoretically, respecting the kind of influence the tube will exert on the aërial column.

It is assumed that the tube is capable of vibrating so that its particles move in planes perpendicular to the axis, with the same motion in all directions from the axis in the same transverse section. Then, if the vibrations of the tube be of very small extent, and its diameter small, compared with its length, the following are the principal mathematical results respecting the motion of the air, so far as it is consequent upon the vibrations of the tube.

1. The motion of the particles situated on the axis will take place in the direction of the axis, and will be nearly the same as if an impulse were originally given in this direction, and the propagation were rectilinear.

2. At all points of the same transverse section, the motion, estimated in a direction parallel to the axis, will be nearly the same.

3. If the tube be made to vibrate isochronously, and so as to contain, at equal intervals along its length, nodal sections and sections of maximum vibration, it will produce in the fluid vibrations of the same duration, with points of quiescence and of maximum vibration at intervals corresponding to vibrations of that duration in *air*.

4. But unless the nodal sections of the tube be fixed, the duration of these simultaneous vibrations will not be permanent till the intervals between the nodal sections become the same in the tube as in the column of air; and then a nodal section of the tube is nearly coincident with a section of maximum vibration of the fluid.

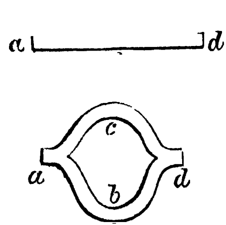
From these results it follows that there are certain transverse vibrations of the tube which will impress on the fluid column the same kind of motion as it is known can be given to it by vibrations excited near one extremity of the tube when the other is open. Mathematicians have succeeded in satisfactorily representing the circumstances of the motion in the latter case of disturbance, by assuming, from experiment, that the open end is a position of maximum vibration, or nearly so; but hitherto no distinct cause for this

fact has been assigned. Mr. Challis thinks it may be shown mathematically that the aërial vibrations, excited at the extremity of the tube, and propagated along its interior, will put it into the state of vibration, which, as appears from the foregoing results, will produce an effect the same in *kind* as that observed. But to what *degree* the phænomenon may be attributed to this cause, can be learnt only from experiment, by ascertaining whether the vibrations of the tube have any considerable influence on the intensity of the musical sounds. The following fact seems to favour the idea of a sensible influence. A sound produced under glass, (for instance, the ticking of a French clock under a glass covering,) is *louder* than when the glass is removed, plainly by reason of the internal reflexions and the propagation of the vibrations along its surface, which cause it to vibrate so as to act with increased effect on the external air. It is not easy to discern that the glass vibrates, but the increase of sound is proved to be owing to this cause, when, on pressing the glass with the palms of the hands, the intensity is diminished. This experiment may suggest the means of detecting the influence of the vibration of a solid, in other instances of a similar nature.

*Case of Interference of Sound.* *By* ROBERT KANE, *M.D.*, *M.R.I.A.*, &c.

Among the experimental proofs of the neutralization of waves, suggested by Sir John Herschel in his interesting paper on the absorption of light, is one which consists in transmitting through a system of canals, waves of sound, emanating from one origin, and reuniting after that by the route of one having been rendered more circuitous than that of the other, when the difference in the lengths of the paths has become such as to qualify them for interference. It occurred to Professor Kane to ascertain whether Sir J. Herschel's idea could be verified in practice, and in certain cases the result has been found satisfactory.

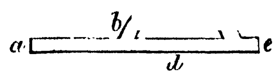
A system of tubes was constructed in which the lengths of the paths were as two to three. Thus in the annexed figures (which, notwithstanding the difference of shape, produced precisely the same results,) the shorter path *a. c. d.* is as 10 inches and the longer *a. b. d.* 15 inches in length. The waves of sound were generated by the languette mechanism of an organ-pipe applied at *a.* or *d.*, and the series obtained first for each tube separately, and then from the system of both. The series of the shorter tube was found $E'. E''. B''. E'''.$, and that of the longer tube $A. A'. E''. A''. C'''. E'''.$




When the tubes were sounded together, the latter series was obtained complete, and the notes of the shorter tube completely sup-

pressed. It was found, however, that the sounds of the longer tube, which also belong to the series of the shorter, were obtained with superior clearness, as E''. E'''. and A''. and B''. appeared to break into each other.

Other experiments having shown that systems of tubes may, by certain methods of vibration, be forced to produce sounds not included in their natural series of harmonics, and it being possible that the suppression of the proper vibrations of the shorter tube resulted not from the ordinary principle of interference but from being forced into unison with the longer one, Professor Kane endeavoured to obtain a system in which the whole series of neither tubes should be suppressed, but that certain notes should be absorbed from the series of each. In only one case did he succeed, but in that one the result is very satisfactory. A combination was made of this figure, in which the length of the path *a. b. c. e.* was 21 inches, that of the path *a. b. d. e.* was 18 inches. The series of the shorter tube




was F. F'. C''. F'''., and of the longer D. D'. A'. D''. F''. A''. D'''. The waves being excited from the orifice *c.* the series of the system was D. F. D''. F''. A''. C'''. Hence the notes F'. and C''. had been absorbed from the series of the shorter, and the notes D'. and A'. from that of the longer tube: whilst the F. F''. and C'''. of the one and the D. D''. and A''. of the other tube maintained their place in the series given by the system.

## *On the various Attempts which have been made to imitate Human Speech by Mechanical Means. By Professor* WHEATSTONE.

Professor Wheatstone gave an account of the various attempts which have been made to imitate the articulations of speech by mechanical means. He described and repeated the experiments of Kratzenstein, De Kempelen, the Abbé Mical, and Mr. Willis of Cambridge. De Kempelen's speaking-machine was exhibited in the course of the lecture, and made to pronounce many words and a few short sentences. Professor Wheatstone concluded with an analysis of the elements of speech founded on these and other investigations, and pointed out the importance of the inquiry as connected with philology.

## *On the Construction of Public Buildings in reference to the communication of Sound. By Dr.* D. B. REID.

Dr. Reid maintained, from numerous experiments made in the open air in the neighbourhood of Edinburgh, in which he was assisted by a number of gentlemen, and also from a comparison of his class-room with many other buildings, that any difficulty in the communication of sound in large rooms arises generally from the

interruption of sound produced by a prolonged reverberation, and, comparatively, rarely from a deficiency in the voice of the speaker. The human voice had been heard distinctly at the distance of a mile and upwards in a calm atmosphere. Sir John Ross, Lieutenant Bowen, and many others, had borne testimony to this fact; and in the experiments above alluded to, in the open air, individuals conversed easily at a distance varying from 200 to 1000 feet, when it was calm. In many rooms, in consequence of the repeated reflections between wall and wall, or between roof and floor, the sound of the voice might be heard continued many seconds after the individual trying the experiment had ceased to speak. In a newly fitted-up leaden chamber for the manufactory of oil of vitriol, the sound was heard prolonged for seven seconds; and when the different notes of any chord were sounded successively by any individual, they were afterwards heard blending harmoniously in one compound tone. The leaden chamber was 80 feet long, 15 broad, and 16 high. In numerous public buildings similar effects are observed; but if the walls be made rough and irregular, so as to lose all resilient power, and hung with drapery, the reverberation ceases. On the same principle, the reflecting power of the floor being taken away by a crowded audience, sound is very different in such an apartment from what is observed when it is comparatively empty. The distinction between the actual amount of sound and purity of intonation has not been sufficiently attended to. Much sound may be produced when the primary impulse is strengthened by combination with the reflected sound of many preceding words, but it has none of that harmony and distinctness which is observed when the primary sound alone is allowed to fall upon the ear. The sound of cannon has been heard at the distance of 300 miles. Captain Stoddart's account of the firing of cannon in the Baltic heard at this distance, affords the most ample and specific information on this subject. The sound of volcanic eruptions has been heard at a distance of nearly 900 miles. It cannot be doubted that the repeated reflection of preceding sounds must interfere most materially with those that succeed; and, from what has been above stated, it is obvious that such reflections must be continued frequently to a great extent in numerous apartments.

In constructing buildings, the following circumstances require to be particularly noticed.

In the most perfect form of building for the communication of sound, any reflected sound must be prevented from continuing so as to interrupt any new tone, by being thrown upon a non-reflecting floor. So long as the reflected sound comes up in time to strengthen the primary impulse before any new sound is heard, it is to be taken advantage of; beyond this it is injurious. A building having low walls, rough and irregular on the surface, an inclined roof terminating in a ridge in the centre, and having any elevation there that might be necessary, the material of which it is made having great reflecting power, with a floor matted and arranged so

so as to absorb all pulses of sound, would be best adapted for this purpose.

All superfluous space should be excluded.

The air should be maintained as uniformly equal as possible.

All concave surfaces ought to be avoided; foci, in such cases, collecting the sound at one point, while in other places it is comparatively deficient.

Dr. Reid, after alluding to the peculiarities in the construction of his class-room, and to many other buildings, adverted to a number of circumstances connected with the roof, walls, and floor of different buildings, the introduction of ornaments, the variety of form that might be adopted according as the walls, roof, &c. were made to reflect or absorb sounds, and the different conditions to be attended to where the speaker was confined to one spot, and where individuals rose in every place to address an assembly.

## *Experimental Researches into the Laws of the Motion of Floating Bodies. By* J. S. RUSSELL.

It was the object of these inquiries to assist in bringing to perfection the theory of Hydrodynamics, and ascertain the causes of certain *anomalous facts* in the resistance of fluids, so as to reduce them under the dominion of known laws.

The resistance of fluids to the motion of floating vessels is found in practice to differ widely from theory, being, in certain cases, double or triple of what theory gives, and in other and higher velocities, much less. These deviations have now been ascertained to follow two simple and very beautiful *laws*: 1st, A law giving a certain *emersion* of the body from the fluid as a function of the velocity. 2nd, A law giving the resistance of the fluid as a function of the velocity and magnitude of a wave propagated through the fluid, according to the law of Lagrange. These two laws comprehend the anomalous facts, and lead to the following

### *Results.*

1. That the resistance of a fluid to the motion of a floating body will rapidly increase as the velocity of the body rises towards the velocity of the wave, and will become greatest when they approach nearest to equality.

2. That when the velocity of the body is rendered greater than that due to the *wave*, the motion of the body is greatly facilitated: it remains poised on the summit of the wave in a position which may be one of stable equilibrium; and this effect is such that at a velocity of 9 miles an hour the resistance is less than at a velocity of 6 miles behind the wave.

3. The velocity of the wave is independent of the *breadth* of the fluid and varies with the square root of the *depth*.

4. It is established that there is in every navigable stream a certain velocity at which it will be more easy to *ascend* the river against the current than to *descend* with the current. Thus, if the current flow at the rate of one mile an hour in a stream 4 feet deep, it will be easier to *ascend* with a velocity of 8 miles an hour on the wave than to *descend* with the same velocity behind the wave.

5. That vessels may be propelled on the summit of waves at the rate of between 20 and 30 miles an hour.

### *On a Species of Balance and its Application to the Measurement of Electrical Repulsion. By* W. SNOW HARRIS.

The principle of this instrument depends on the reactive force imparted to two parallel silk threads without torsion, from which is suspended a horizontal needle or other body. If a needle be suspended by two parallel and vertical filaments of silk without torsion, equally distant from the centre, its position of rest will be horizontal, and in the vertical plane passing through the silk filaments. When the needle is turned through any given angle, the centre of gravity of the mass is raised, so that the needle will, when abandoned to the force of gravity, continue to oscillate, and will be in the state of a body falling down a small circular arc. Mr. Harris has examined the law of this force imparted to the threads, and finds it as the weight and square of the distance between the threads directly and as the length indirectly, and that it is exactly proportionate to the angle of deflection of the needle. Upon these principles Mr. Harris has constructed a balance, which he exhibited to the Section, and by which he can estimate any forces of repulsion in electricity however small. The instrument is not liable to many difficulties which embarrass the use of the torsion balance, and may be employed with advantage in several branches of experimental physics.

### *On Electrical Attraction. By* W. SNOW HARRIS.

The object of this paper was to examine the operation of attraction in electricity, and the laws and differences between the force of attraction actually exerted between two bodies, and the force excited in a neutral uninsulated body, by the influence of a charged body acting upon it at a distance. The author endeavoured to show that the former force varied in an inverse ratio of the distance simply; that the law of the inverse square of the distance, which is the general law for the former force, does not obtain at all distances between bodies, except one of them be uninsulated and neutral; and that in all cases of attraction there are two previous forces to be considered, 1st, the force directly induced in the neutral body; 2nd, the effect of this induced force upon the charged body; which last he called

the reflected force, and attempted to prove that the whole attractive force between these bodies varies with these forces conjointly, so that if one of them becomes fixed it varies with the other. He exhibited and described several new experiments in electricity relating to electrical induction and attraction, and expressed his opinion that the whole attractive force was dependent on the action excited in the neutral bodies reflected on the charged body. This principle, with but little modification, he further applied to any case of electrical attraction whatever.

*On the Application of the Proof Plane and Torsion Balance to inquiries in Electricity. By* W. SNOW HARRIS.

Mr. Harris conceives that an insulated plate of metal of small thickness may take up unequal quantities of electricity from a body and yet the distribution be uniform. The experiments in illustration of this were fully discussed. He alluded to several laws of electrical intensity attendant on the disposition of electricity on surfaces and plates varying in extension and in length, but of the same area, and endeavoured to show that contrary to the ordinary view of electrical distribution, electricity existed on both surfaces of a hollow sphere, as well as on both surfaces of a plate of the same area. He considers every case of attraction in electricity to resolve itself into the case of charging a coated non-conducting body, and that the phænomena always correspond to those observed in the latter.

*On the Aurora Borealis. By Sir* JOHN ROSS.

Having observed in his first arctic expedition that the aurora sometimes appeared between the two ships, and also between the ships and the icebergs, and found in his subsequent experience, both in Scotland and during the second arctic voyage, proofs satisfactory to his own mind that the aurora takes place within the cloudy regions of the earth's atmosphere, Sir John Ross states the following hypothesis on the subject, viz. "The aurora is entirely occasioned by the action of the sun's rays upon the vast body of icy and snowy plains and mountains which surround the poles."

*On an œconomic Application of Electro-magnetic Forces to manufacturing Purposes. By* ROBERT MALLET.

The separation of iron from brass and copper filings, &c., in workshops, for the purpose of the refusion of them into brass, is commonly effected by tedious manual labour. Several bar or horse-shoe magnets are fixed in a wooden handle, and are thrust, in various directions, through a dish or other vessel contain-

ing the brass and iron turnings, &c., and when the magnets have become loaded with iron it is swept off from them by frequent strokes of a brush. This is an exceedingly troublesome and inefficacious process.

It appeared to the author that a temporary magnet of great power, formed by the circulation of an electric current round a bar of iron, might be substituted advantageously. The following is the arrangement which he has adopted. Several large round bars of iron are bent into the form of the capital letter U, each leg being about six inches long. They are all coated with coils of silk-covered wire, in the usual way of forming electro-magnets of such bars, and are then arranged vertically, at the interval of five or six inches from each other.

All the wires from these coils are collected into one bundle at their respective poles, and there joined into one by soldering, a large wire being placed in the midst of them and amalgamated. A galvanic battery is provided, which, if care be taken in making the junctions at the poles, &c., need not exceed four or at most six pairs of plates, of from twenty inches to two feet square. The poles of this terminate in cups of mercury, which are so placed that the large terminal wires of all the coils can be dipped into them, or withdrawn easily.

The rest of the arrangement is purely mechanical. The required motions are taken from any first mover, usually a steam engine. The previously described arrangement being complete, a chain of buckets is so contrived as to carry up and discharge over the top of the magnets a quantity of the mixed metallic particles: most of the iron adheres to the magnets, while the so far purified brass falls into a dish or tray placed beneath to receive it. This latter is also one of a chain of dishes, the horizontal motion of which is so regulated that the interval between two dishes is immediately under the magnets, in the interval of time between two successive discharges of the mixed particles on the bars.

At this juncture the communication between the galvanic battery and the magnets is interrupted by withdrawing the wires from the cups of mercury, and the result is, that the greatest part of the adhering iron drops off and falls in the space between the two dishes. The next dish now comes under the magnets, the communication is restored, and a fresh discharge from the buckets takes place, and so the process is continued.

Some iron constantly adheres to the magnets, but this is found of no inconvenience as it bears but a small proportion to the total quantity separated.

The author has had an imperfect apparatus of the sort above described at work for some time, and has found it to answer; and suggests the application of electro-magnets for somewhat analogous objects in various manufactures. He particularly mentions needle and other dry grinding.

*An Inquiry into the Possibility and Advantage of the Application of Magnetism as a Moving Power, with Remarks on the Nature of Magnetism. By the Rev.* James William M'Gauley.

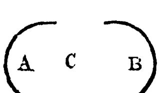
To consider with advantage the possibility of applying magnetism as a moving power, we must examine its nature and peculiar properties, because otherwise we cannot pronounce with accuracy on the quantity at our command, or the probable cheapness of its production. In this inquiry the author does not contemplate such a power as that attained in magnetic rotations and similar mechanism; it could never be advantageous; for the force of the magnet is not directly applied, or is applied at such a distance as to be almost annihilated.

The quantity of magnetism we *may produce* seems to have no limit, since we can combine any number of powerful magnets.

*The œconomy of magnetism.* A very small electrical power, which may be produced if necessary by the agency of sea-water, will abundantly suffice.

*The obstacles* likely to prevent the application of magnetism as a moving power. Of these the principal seems to consist in the disturbing influence which magnets of any power exercise over each other. This prevents the necessary reversion of the poles.

Experiment 1st. The author tried to reverse the poles of one electro-magnet in contact with another: the sudden rush of electricity evidently caused a magnetic needle near the magnet to be affected, but there was no separation or repulsion of the magnets, nor any permanent change of polarity.—Experiment 2nd. The similar poles of two electro-magnets of very different power were brought together: they attracted each other; the poles of the smaller magnet were reversed by the larger, and a counter-current was formed through its battery, and indicated by a galvanometer placed in the circuit.—Experiment 3rd. Only one of the magnets was excited, then its poles reversed; the other, acting as a keeper, was thrown off, and attracted with great violence.—Experiment 4th. Between two semicircular magnets A and B, a bar of soft iron, C, was suspended, and their poles reversed in such a manner alternately as to throw off the bar from one magnet and cause it to be attracted by the other.

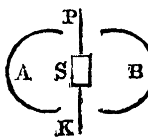


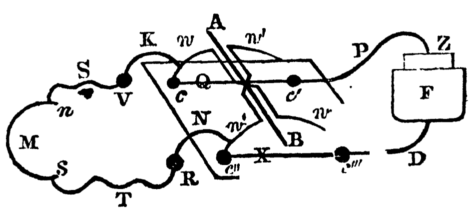

—Experiment 5th. A bar of magnetized steel was placed between the magnets; but the effect was not so powerful, since the iron bar became by induction a stronger magnet than the steel, and hence the mutual actions of the iron bar and the magnets was more powerful.

The very limited space within which magnetic action is confined presents a very considerable obstacle. The power is inversely as the square of the distance; at the eighth of an inch the power even of a large magnet is comparatively trifling. The stroke of one eighth of an inch, directly applied to machinery, would be nothing: we must increase the stroke, and at the same time diminish the power as little

as possible. If we increase the stroke, by increasing the distance between the bar and the magnets, we diminish the power inversely as the *square* of the distance; if by applying the power of the magnet at the shorter arm of a lever, we diminish only in the inverse ratio of the *distance*: thus, if it is wanted to increase the stroke twelve times, the power in one case with the smaller distance, is to the power in the other with the larger distance, as the square of the larger is to the square of the smaller, as $12^2 : 1^2 :: 144 : 1$. With a lever, the power with the smaller distance is to the power with the larger as the distance in the latter case is to the distance in the former, $:: 12 : 1$. The power in any case is much diminished; but as we can create it in any quantity, this is of little consequence. The repulsion of the magnet for the bar, though considerable, is much less than its attraction.

The construction of the machine by which Mr. M'Gauley has exemplified the application of magnetism as a moving power is easily conceived. An oaken frame supports two magnets, A and B, horizontally. The bar P K, fixed in a strong pendulum of wood, of which S is a horizontal section, swinging on steel knife-edges, vibrates between the magnets, and has attached to its lower extremity a rod connected with the reversing apparatus and any other required machinery. The poles of the magnets are simultaneously reversed, and the bar driven with great force from one to the other, and with a velocity of two or three hundred vibrations in a minute. The apparatus for reversing the poles is simple, and can be adapted almost without increasing its weight to any combination of magnets. Let A B represent the axis upon which the wires $w$ $w$ and




$w'$ $w'$, crossing each other under it, are turned; these dip into cups of mercury, $c$ and $c'$, connected with each other by the wire Q, and with the zinc plate of the battery by the wire P; and into the cups $c''$ and $c'''$, connected with each other by the wire X, and with the copper of the battery by the wire D. $w$ $w$ and $w'$ $w'$ are connected with the cups V and R by the wires K and N, and with the poles of an electro-magnet M by the wires S and T. The wires K and N rise in the cups of mercury V and R, but do not leave the mercury. Let us suppose, as in the figure, the wires $w$ $w$ and $w'$ $w'$ to dip into the cups $c$ $c''$: we shall trace the electricity. It flows

from the copper of the battery F along D to $c'''$, along X to $c''$, along N to R, along T to S, where it enters one pole of the magnet M. Now, let the wires *w w* and *w' w'* have turned a little on their axis A B, so as to dip respectively into $c'$ and $c'''$. The electricity flows from F along D to $c'''$, along *w w*, crossing the axis A B, along K, to V, along S, and enters the magnet at N, before it entered at S; hence the current is inverted and the poles reversed.

The machine can be stopped or set in motion in a moment by lifting or replacing any of the wires forming the galvanic circuit. If its motion be interrupted its power is not wasted as in other machinery, but is accumulating; so that when it again works, it acts with increased power and velocity. We can continue the most perfectly uniform motion for any length of time by allowing additional fluid to drop very gradually into the copper of the battery; the one copper will answer for any number of magnets. If the zinc plates be separate, by insulating these plates with flannel bags we greatly increase the power and add to the duration of the galvanic effect.

Mr. M'Gauley has endeavoured to examine the relative lifting power of magnets of various forms. The following are some of the results:

*The Iron.* Three helices of the same wire, each 22 feet in length were coiled on three different magnets, and the same battery was used.

| | | lb. | oz. |
|---|---|---|---|
| Magnet No. 1. | In length 28 inches, diam. 2 inches, power | 6 | 8 |
| 2. | ——— 8 ——— $\frac{5}{8}$ ——— | 11 | 11 |
| 3. | A magnet having knobs, B and C each $1\frac{3}{16}$ inches in length, diameter 1 inch, and connected by an arm $\frac{1}{16}$ by $\frac{7}{16}$, the wire coiled on the knobs, with connecting spiral; power - - - | 2 | 4 |


*It was found on another occasion that when the helix did not bear* so great a disproportion to the iron, the power of the larger magnet was comparatively very great. To learn the best size for the iron bar suspended in the pendulum of the machine, three forms of keeper were tried.

| | | | |
|---|---|---|---|
| No. 1. | $5\frac{3}{4}$ long, $\frac{5}{16}$ thick, $\frac{1}{4}$ wide; power ............ | 4 | 0 |
| 2. | $5\frac{1}{16}$ —— $\frac{5}{16}$ —— $\frac{1}{2}$ —— —— ............ | 3 | 8 |
| 3. | $5\frac{7}{8}$ —— $\frac{5}{16}$ —— $\frac{1}{4}$ —— —— ............ | 7 | 8 |

The large magnet would not lift a steel needle, but lifted a wire of soft iron equal in size to the needle. Perhaps the intensity of the magnetism, though its sum was nearly equal with the same coil, was smaller when diffused through the particles of the larger magnet, and was unable to disturb the magnetic equilibrium of the steel needle.

*The Battery.* The magnet designated above as No. 2. was tried with a battery:

| | lb. | oz. |
|---|---|---|
| No. 1. Double cell, each 1 foot square; power ........ | 11 | 11 |
| 2. ——— 1 foot by 6 inches ................ | 6 | 11 |
| 3. Single cell, 3 inches by 2½ ................ | 0 | 8 |
| 4. ——— 1 inch square .................... | 0 | 2 |

Same magnet with single cell of battery No. 2, and charged with

| | | |
|---|---|---|
| No. 1. 1 part sulphuric acid, 50 parts water; power.... | 5 | 0 |
| 2. 1 —— nitric acid, 50 ———— .... | 3 | 8 |

The spark with these charges was at first very brilliant, but the effect was transitory.

| | | |
|---|---|---|
| No. 3. Diluted alcohol, 10 parts in 100; power........ | 0 | 4 |
| 4. 2 parts sulph. acid, 1 nitric acid, 100 water .... | 11 | 10 |
| 5. 1 part ——— 2 ——— 100 —— .... | 4 | 0 |
| No. 4. was tried with a helix interposed between the positive pole of the battery and the magnet. The lifting power was .................................. | 1 | 0 |
| Another magnet being interposed, power ............ | 3 | 0 |
| When No. 4 was exhausted, so that it would lift only 1 pound, its zinc plate was raised out of the fluid and replaced; this increased the power to .............. | 2 | 8 |
| When No. 4 would lift only 1 pound its fluid was poured into its other cell, and the power became .......... | 5 | 9 |
| 8 pairs of plates in Cruikshank's battery caused magnet No. 4 to lift .................................. | 5 | 8 |
| A calorimotor of equal surface, charged with a similar fluid | 11 | 10 |

Perhaps the increased tension of the electricity might be found of advantage when the helix is of great length.

In speaking of the charge, the author remarks that he is fully persuaded of the necessity of decomposition for the production of galvanic effect. A deflexion of 15° and more has been produced in a galvanometer of great delicacy, which he constructed by merely uniting, by a single corner of each, two tarnished pieces of metal, the one zinc, the other copper; the imperceptible perspiration of the hand may have acted as a fluid, and some foreign substance deposited on either or both metals have aided the decomposition.

*The Helix.* Four magnets with different helices were used with the same battery.

| | | |
|---|---|---|
| Magnet No. 1. 8 inches long, ⅝ diam., coil 7½ yards, power | 11 | 0 |
| 2. ———————— 4 —— —— | 6 | 0 |
| 3. That with knobs already mentioned, coiled with 7½ yards of wire ................ | 2 | 5 |
| Coiled with ribbon of copper on the knobs .. | 0 | 2 |

Five magnets, each 8 inches long and ⅝ diameter, were coiled

with 7¼ yards of wire, and the same battery was used with each magnet.

| | | |
|---|---|---|
| No. 1. The wire was coiled only on the ends, and crossed straight from one pole to the other .......... | 1 | 0 |
| 2. Wire coiled on the ends, but connected by a spiral round the magnet ........................ | 11 | 0 |
| 3. Wire divided, and each half placed as a helix on one end of the magnet .................... | 6 | 0 |
| 4. Wire coiled equally over the whole magnet ...... | 7 | 0 |
| 5. Wire divided into 4 equal parts, each coiled on one fourth of the magnet ...................... | 5 | 0 |
| 6 yards of wire were coiled on a magnet 7¼ inches long, ½ square ........................ | 7 | 0 |
| 1½ yard of wire were coiled on a magnet 7½ inches long, ¼ square ........................ | 3 | 0 |

Hence the power of the magnet increases far more rapidly than proportionately with its size.

*Remarks on the Nature of Magnetism.* The author in this part of his paper discusses the prevalent theories of magnetism, and compares them with a variety of experiments corresponding to the analysis which he presents of the subject. It would be nearly impracticable to do justice to Mr. M'Gauley's views on the nature of magnetism in the compass of an abstract. The following brief notice will serve to convey some idea of his mode of reasoning.

Magnetism does not arise from the circulation of electrical currents, but from the electrical excitation of the mass or the particles in the magnet: not from currents, because it can begin to exist without them, can continue to exist without them, and because currents can be generated in conducting substances of sufficient quantity, velocity and intensity, without the development of magnetism. Magnetism is mere electrical excitation, provided that by mere electrical excitation we can cause its existence, and its various phænomena are such as we should expect from mere electrical excitement; and provided not electrical currents but electricity at rest be always coexistent with it. Such, the author contends, are the facts, and he proceeds to prove his position by appropriate experiments.

He then offers explanations in agreement with these views of several leading phænomena, as terrestrial induction, the mutual attraction of conjunctive wires, the position of the poles, of a permanent magnet, and of an electro-magnet, the retention of magnetism in steel, the destruction of magnetism by heat, the development of it by percussion, &c. He finally observes:

If magnetism be merely electrical excitation it is probable that, cheap as their production is at present, a more œconomical mode of forming powerful magnets may yet be discovered. Though it may be said that magnetism is not now for the first time applied to machinery, the author believes it will be acknowledged that the attempt to apply magnetism to machinery, as an advantageous and a powerful

agent, has been totally unsuccessful. In the experiment brought under the notice of the Section, the velocity with which the poles of any number of magnets are reversed is inconceivable, and the whole lifting power is applied with the greatest possible advantage directly to the mechanism; circumstances which appear to justify the author's expectation that the application of magnetism to machinery, as a moving power, will ultimately be successful.

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Description of a New Dipping Needle. By R. W. Fox.

[A description of this instrument has been already printed by the author.]

Abstract of Hansteen's Researches in Magnetism. By Capt. SABINE.

[This paper will be printed in the next volume of Transactions.]

Account of Magnetic Observations in Ireland. By Capt. SABINE and Prof. LLOYD; communicated by the latter.

[This will be printed in the next volume of Transactions.]

Results of three years and a half hourly Observations with the Thermometer at Plymouth. By W. SNOW HARRIS.

[This Paper will be printed in the next volume of Transactions.]

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*On the Measurement of Heights by common Thermometers. By* Lieut.-Col. SYKES.

The thermometric instrument for measuring heights invented by the late Rev. F. J. H. Wollaston, though very sensible, has been found by the author and Mr. James Prinsep of Calcutta too fragile and too expensive for *rough work*. After the destruction of three of these instruments, Col. Sykes had recourse to common thermometers, which, with certain precautions, he found to answer extremely well, and having tested their indications by contemporaneous barometrical observations, he was enabled to present a table of comparative results. The thermometer to be observed was uniformly kept in the water, which was made to boil violently, about 2 inches above the bottom of the pot; two thermometers were successively employed, the difference of their scales being known; different tables of the elastic force of steam were employed in the reduction; and from the whole of the results the author has collected a few into a table, calculated to show the limits of error, of thermometric measures of heights in India, when the boiling temperature of the level of the sea is *assumed to be constantly* 212°; of single barometrical observations, when the pressure at the level of the sea is *assumed to be* 30,000 inches, (mean temperature in both cases 82°;) and of *corresponding* barometrical observations. The general agreement of all the results, by whatever method obtained, is remarkable, and is considered by Col. Sykes as justifying his opinion that common thermometers may be satisfactorily used to supply the place of barometers in measuring heights, where great accuracy is not required.

| Year. | Date. | Names of Places. | ALTITUDES DEDUCED FROM 1. Jones's Bar. No. 2, with assumed pressure of 30° in., and mean temp. 82° at the level of the sea. | 2. Cary's Barometer, No. 2, ditto ditto. | 3. Corresponding observations with Capt. Jervis's Gilbert's Bar. and Cary's No. 2. | 4. Corresponding observations with Dr. Walker's Gilbert's Bar. and Cary's No. 2. | 5. Corresponding observations with Capt. Jopp and Cary's Bar. No. 2. | 6. Corresponding observations with Cary's Bars. Nos. 1 and 2. | 7. Corresponding observations with Cary's Bar. No. 1, and Jones's No. 2. | 8. Difference of boiling temperatures, therm. 1 by | 9. Difference of boiling temperatures, therm. 2 by | 10. Difference of boiling temp. by Dr. Robinson's tables, Indian Navy. | 11. Difference of boiling temperature, by tables of James Prinsep, Esq., Calcutta. | Difference. | 12. Mean of corresponding observations by Barometers. | 13. Mean of boiling temperatures. |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 1827. | 23 May...... | Highest point, Hill Fort of Poorundhur.................... | 4588 | 4599 | ...... | ...... | ...... | †4471 | 4528 | 4536 | 4553 | 4415 | 4427 | −16 | 4499 | 4488 |
| 1827. | 10 May...... | Singhur Hill Fort .................... | 4199 | 4180 | ...... | ...... | ...... | †4211 | 4170 | 4341 | 4220 | 3927 | 3928 | −86 | 4190 | 4104 |
| 1828. | 15 May...... | Temple at Beema Shunkur ...... | ...... | ...... | ...... | ...... | †3090 | ...... | ...... | 3037 | 3037 | 2992 | 2991 | −71 | 3090 | 3019 |
| 1825.<br>1827. | 6 March...<br>11 May..... | Karleh, or Carlee Cave Temple.. | 2493 | 2652 | ...... | ...... | ...... | †2530 | ...... | 2693<br>2526 | 2616<br>2526 | 2468 | 2478 | +27 | 2530 | 2557 |
| 1827. | 23 May....... | Highest point of Poorundhur above Poona ........ | 2697 | 2681 | ...... | ...... | ...... | †2648 | †2650 | 2661 | ...... | 2539 | 2566 | −61 | 2649 | 2588 |
| 1828. | 9 Feb....<br>3 April.. | Part on the Yail River.............. | ...... | ...... | †2478<br>†2493 | ...... | †2470 | ...... | ...... | 2494 | 2494 | 2480 | 2484 | +8 | 2480 | 2488 |
| 1828.<br>1829. | ................ | Temple in the Hill Fort of Hureechundurghur ......... | 3972 | 3931 | 3845 | †3922 | †3871 | †3887 | 3935 | 3840<br>3887 | 3869<br>3887 | 3824 | 3788 | −46 | 3892 | 3846 |
| 1829. | 11 to 17<br>Decem.. | Source of Kristna River at Mahabuleshwur ............ | ...... | ...... | ...... | †4496<br>†4503 | ...... | ...... | ...... | *4498 | *4556 | *4422 | *4425 | −24 | 4499 | 4475 |
| 1828. | 27 April..... | Pokree ................................ | ...... | ...... | ...... | ...... | †3197 | ...... | ...... | 3194 | 3194 | 3185 | 3141 | −19 | 3197 | 3178 |
| 1828. | 6 April...... | Kullumb, on Goreh River......... | ...... | ...... | 2043<br>2027 | ...... | †1995 | ...... | ...... | 1971 | 2000 | 1988 | 1986 | −36 | 2022 | 1986 |
| 1825.<br>1826.<br>1827.<br>1828.<br>1829. | ................. | Poona, Hay Cottage.................. | ...... | ...... | 1810<br>1820 | †1810 | †1837 | †1823 | ...... | Means<br>1883 | 1897 | 1876 | 1861 | +59 | 1820 | 1879 |
| 1828. | 16 Feb......... | Downde, on the Beema River..... | ...... | ...... | ...... | ...... | 1623 | ...... | ...... | 1591 | 1591 | 1567 | 1575 | −41 | 1623 | 1582 |
| 1828. | 29 Oct......... | Seaswur, above Poona ............... | ...... | ...... | ...... | 592 | ...... | ...... | ...... | *511 | *456 | ...... | ...... | −107 | 592 | 485 |

† The heights most relied upon.

* Boiling temperatures determined by Dr. Walker.

Results of a Third Series of Experiments on the Quantities of Rain received at different Heights in the Atmosphere. By W. Gray, Jun. and Prof. Phillips.

[This Paper will be printed in the next volume of Transactions.]

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Dr. Apjohn explained the substance of two papers recently read by him before the Royal Irish Academy, and which have within a few days appeared in the last part of their Transactions. In the first of these papers a formula is investigated for pointing out what has long been considered a desideratum in meteorology, namely, the exact relation between the indications of the wet-bulb thermometer and the corresponding dew-points; while, in the second, a number of experiments are detailed, instituted for the purpose of testing its accuracy, and which seemed to prove that the formula represented observations with an extreme precision. The following is an outline of his communication, which was made orally to this Section.

When the wet thermometer attains its stationary temperature, the caloric which it loses and acquires in a given time are obviously equal. The latter is that imparted by the surrounding air to the instrument in cooling through $t - t'$ † degrees, and the former that which constitutes the caloric of elasticity of the vapour formed. Now if $m$ be the amount of moisture which a given weight of air is capable, when saturated, of containing at the temperature $t'$, and $m'$ the quantity of vapour which would be formed at the same temperature by the caloric evolved from the air in cooling through $t - t' = d$ degrees, then the relation in question is expressed by the equation $f'' = f'\left(\frac{m - m'}{m}\right)$ in which $f''$ is the tension of vapour at the dew-point, and $f'$ its tension at the temperature of the wet-bulb thermometer. This expression is rigorously exact, for in arriving at it we merely assume what must at once be conceded, namely, that the air which is cooled by contact with the moist bulb becomes saturated with moisture at the temperature $t'$, and that the tension of vapour at a given volume and a given temperature is directly proportional to its quantity or specific gravity. But the value of $m$ is easily assigned by aid of the theory of mixed gases and vapours, and that of $m'$ also admits of being readily deduced from the known values of the specific heat of air and the caloric of elasticity of vapour. When this is done, and the proper substitutions made, the above expression is converted into the following: $f'' = f' - \frac{d}{87} \times \frac{p}{30}$.

To this solution of the dew-point problem it may be objected that the coefficient which is set down as $\frac{1}{87}$ cannot be constant, in as much as its value depends upon the latent heat of aqueous vapour and the specific heat of the medium which encompasses the wet-thermometer, both of which vary, the former with the temperature, and the latter with the pressure and the amount of vapour present in the air. Such objection is theoretically just, and the necessary corrections have therefore been investigated by the author in his

original paper, and may be applied if deemed necessary. Experience however has satisfied him that, generally speaking, they may be neglected, as in almost every instance their amount is considerably within the inevitable errors of observation.

The experiments instituted for the purpose of testing the formula, and which are detailed in the author's second paper, were next explained. The principle of the first of these is as follows: if air, in reference to which $t$, $t'$ and $t''$ (the dew-point) have been accurately noted, be raised to any elevated temperature, and the observation be repeated in the heated air as far as respects $t$ and $t'$, we shall have two separate sets of observations from which to calculate the point of deposition; and as the amount of moisture in the air is not altered by the augmentation of temperature it has experienced, both calculations, provided the formula be correct, should give precisely the same result, *i. e.* the dew-point in the first instance determined by observation. Four distinct series of experiments on this plan were performed by means of a very simple apparatus, and though the depressions varied from $4^{\circ}\cdot7$ to $28^{\circ}\cdot5$, the calculated dew-points for each series were found almost coincident, and the differences between these and the observed dew-points were so trifling in amount as to be clearly ascribable to unavoidable inaccuracy of observation.

The next test experiments performed were suggested by the formula itself. If $f'' = f' - \frac{d}{87} \times \frac{p}{30}$, and $f''$ be supposed equal to 0, a condition which can only be fulfilled in perfectly dry air, $f' = \frac{d}{87} \times \frac{p}{30}$, an equation from which we deduce $d = 87 f' \times \frac{30}{p}$. Hence by determining experimentally the depression of the wet thermometer in perfectly dry air we shall be enabled to pronounce upon the validity of the general method under discussion. In order to observe several values of $d$, air forced from a caoutchouc bag was made to pass three times through about two inches of oil of vitriol, and then to traverse a tube containing the dry and wet thermometer, and the indications of these instruments were noted down as soon as the latter assumed its stationary temperature. Of nineteen observations of depression thus obtained, eleven were a little greater, and eight a little less than the calculated results. The mean of the plus errors of the formula was $\cdot28$, and of the minus errors $\cdot4$ of a degree, so that $\frac{\cdot28 - \cdot40}{19} = -\cdot006$ is the mean difference between experiment and calculation deducible from the whole. This singularly close correspondence of theory with experiment is the more satisfactory because as the mean pressure for the nineteen experiments was but a little over 30, and as the air was perfectly dry, neither of the corrections, of which mention has been already made, required to be applied.

The most obvious method of testing the formula, or that which consists in comparing its results with the dew-points experimentally determined, was last noticed. That such criterion may be decisive,

it is necessary, 1st, that the depressions be considerable in amount; 2nd, that the dew-points be accurately known. Now neither of these conditions is fulfilled by the few registers which have been published, the depressions being small, and the observations made with an instrument (Daniell's hygrometer), the difficulty of observing with which is universally admitted. It occurred, however, to the author, that both difficulties might be overcome in the following simple manner: let air saturated with moisture, and whose temperature is therefore its dew-point, be heated, and let the temperature of the heated air be taken, as also that shown by a wet thermometer subjected to the action of a current of it. Then, by the application of the formula, let the dew-point belonging to the two latter observations be calculated, and from a comparison of it with the original temperature of the air, when saturated with humidity, he expected to be enabled to pronounce with confidence upon the value of his method. Twenty-four distinct observations were thus made, the tabulated results of which justify the following conclusions: 1st, that in the case of seven of them the observed and calculated dew-points are almost coincident; 2nd, that the difference in no instance exceeds, and in but one instance reaches, one degree; 3rd, that the mean difference deducible from the whole is but ·35, or about one third of a degree of Fahrenheit.

At the close of this paper two tables are given by the aid of which the application of the formula is rendered extremely simple and expeditious.

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### *On a New Anemometer. By the Rev.* W. Whewell.

The author described the construction and purpose of an anemometer which he exhibited. The object of the instrument is to obtain a record of the total *amount* of the aërial current which passes the place of observation in each direction. The assemblage of such records for any given time will exhibit a *type* of the course of the wind for such time; the mean of such records at the same place for different years will exhibit the *annual type* of the winds for that place, and the comparison of the types of the winds for many different places will throw light upon the general annual movement of the atmosphere. Some of these instruments are now in course of construction, with a view to their being tried in different places, and it is hoped that some account of their working may be produced at the next Meeting of the Association.

### *Account of the Measurement of the Aberdeen Standard Scale. By* Francis Baily.

[This Paper will be printed in the next volume of Transactions.]

## CHEMISTRY.—ELECTRICITY.

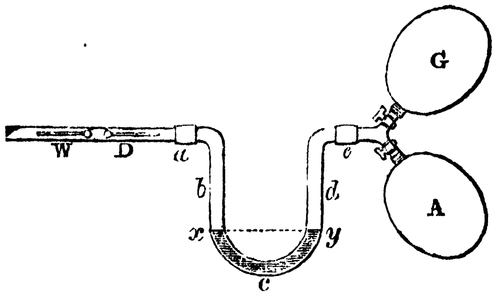
### *On the Specific Heats of the permanently elastic Fluids. By* JAMES APJOHN, *M.D., Professor of Chemistry in the Royal College of Surgeons, Ireland.*

After an introductory view of the state of knowledge on this subject, Dr. Apjohn proceeded to explain the principle of an entirely new method which he was enabled to apply to the investigation of the difficult problem under consideration, in consequence of having been recently fortunate enough to arrive at a formula which expresses, with extreme and unexpected precision, the relation existing between the indications of a wet-bulb thermometer and the corresponding dew-points. This formula* (see Proceedings of Mathematical and Physical Section, p. 27,) being equally true of all gases, obviously suggests a method of comparing their specific heats. For as in the case of every gas it may be deduced that $a = \frac{(f'-f'')e}{48\,d} \times \frac{30}{p}$, it is clear that if we determine values of $f'$, $f''$ and $d$ in different elastic media, we shall have data for ascertaining their relative capacities for caloric. Such a method, however, though theoretically exact, is beset with difficulties so great that it may be considered as practically impossible. The artificial gases, as usually collected, are saturated with moisture, a state in which they are quite unsuited for the necessary experiments; and even though this difficulty were overcome, it would probably be impossible to determine their dew-points by direct experiment.

If, however, we suppose that $f''=0$, or that the gas is perfectly dry, the above value of $a$ will become $\frac{f'\,e}{48\,d} \times \frac{30}{p}$, an expression involving no unknown quantities but $f'$ and $d$, and which will therefore enable us to calculate the specific heat of a gas, when we have observed the stationary temperature $t'$, to which, when in a state of perfect desiccation, it brings the wet-bulb thermometer. In order to the determination of $t'$, and of $t-t'=d$, the following method of experimenting was, after a trial of several others, finally adopted. Into a bent tube, $a\ b\ c\ d\ e$, about 50 inches long, and $\frac{3}{10}$ths of an inch in diameter, oil of vitriol was poured to the height marked by the horizontal line $x\ y$, and to one extremity of this siphon a pair of bladders furnished with stopcocks were attached, through the intervention of a three-armed copper pipe, while to the other extremity of the apparatus there was connected by a caoutchouc collar glass tube carrying the dry thermometer D, and wet one W.

* $f''=f'-\frac{48\,a\,d}{e}\times\frac{p}{30}$, in which $f''$ is the elastic force of vapour at the dew-point; $f'$ its elastic force at the temperature $t'$, shown by the wet thermometer; $d$ the difference between the latter temperature, and $t$ that of the air; $a$ the specific heat of air; and $e$ the caloric of elasticity of the vapour of water whose elastic force or tension is represented by $f'$.

Matters being thus arranged, an assistant pressed, by means of a deal board, first upon the bladder A, containing atmospherical air,




and, when it was exhausted, upon the bladder G, containing the gas which was the immediate subject of experiment. The air, in passing through the oil of vitriol, was deprived of its vapour, and in subsequently traversing the tube containing the thermometers, produced in the *wet* one such a reduction of temperature, that, upon continuing the experiment as rapidly as possible with the *gas*, the wet thermometer soon acquired a stationary temperature,—which, when attained, was, as well as the indication of the dry instrument, carefully noted. The residual gas was now passed into a glass jar on the mercurial trough, with a view to a subsequent analysis; and both bladders being refilled with atmospherical air alone, a second experiment was performed precisely as just described.

From the values of $t$ 0 $t'$ obtained in the first experiment, we obtain, by aid of the equation $a = \frac{e\,f'}{48d} \times \frac{30}{p}$, the specific heat of the elastic fluid which was made to traverse the apparatus. But this result belongs not to the pure gas, but to a mixture of it with a certain quantity of atmospheric air, which entered the bladder upon the principle of endosmose; and to infer from it the specific heat of the pure gas, which we shall call $x$, it was necessary to know, 1st, the amount of air present, and 2nd, *its* specific heat. Now the former of these data was given by the analysis of the residual gas, as already mentioned, and the latter by the results of the second experiment above recorded, in which both bladders were occupied by air alone. If $x$ be the specific heat, and $s$ the specific gravity of the gas, $n$ the per centage of air, $c$ its specific heat, and $a$ the specific heat of the mixture of air and gas, as already determined, we shall, on the principle that the specific heat of a mixture multiplied by its weight is equal to the sum of the products of the weights of the gases mixed multiplied by their respective specific heats, have $x(100-n)s+n\,c = a(100-n\,s+n)$, an equation from which we deduce $x = a + \frac{(a-c)\,n}{(100-n)\,s}$. This is the specific heat of the pure gas in reference to that of air, as determined by the second of the above

experiments; and as both air and gas are dry, and must have been, with at least a very high degree of probability, proportionally affected by variations of pressure, the precise influence of these, about which, indeed, philosophers are not agreed, do not require to be taken into consideration; nor is there anything further necessary for rendering the result thus obtained strictly comparable with those of other experimenters, than to reduce it, by the rule of three, to what it would be if the specific heat of air were ·267, the number by which it is usually represented in books at a mean altitude of the barometer. The following experiments on air and hydrogen, performed on the 4th of August, will illustrate the preceding description.

| | $t$ | $t'$ | $d$ | $p$ |
|---|---|---|---|---|
| Hydrogen...... | 68 | 48 | 20 | 30·114 |
| Air........... | 68 | 43 | 25 | 30·114 |

By applying to these results the equation $a = \frac{f'c}{48\,d} \times \frac{30}{p}$, we get

Specific heat of air .................. =·2767
Approximate specific heat of hydrogen . =·4092

But the gas, upon analysis, was found to contain 5 per cent. of air. Hence the specific heat of the hydrogen supposed pure as deduced from the equation $x = a + \frac{(a-c)\,n}{(100-n)\,s}$ becomes ·5097. And ·2767 : ·5097 : : ·2670 : ·4914 = the specific heat of hydrogen compared to that of air under a pressure of 30,—when water is represented by unity, or, what amounts to the same, when air is ·267.

The following table exhibits the results thus obtained;—referred to air as the standard, the number for nitrogen being the mean of two; that for hydrogen of four; that for carbonic oxide of three; that for carbonic acid of three; and that for nitrous oxide of two experiments.

| | Specific Heats of equal volumes. | Specific Gravities. | Specific Heats of equal weights. |
|---|---|---|---|
| Air............. | 1·0000 | 1·0000 | 1·0000 |
| Nitrogen......... | ·9613 | ·9722 | ·9887 |
| Hydrogen........ | ·1315 | ·0694 | 1·8948 |
| Carbonic oxide.... | 1·0508 | ·9722 | 1·0808 |
| Carbonic acid..... | 1·6677 | 1·5277 | 1·0916 |
| Nitrous oxide..... | 1·7802 | 1·5277 | 1·1652 |

A bare inspection of this table would seem to justify the conclusion that, with a single exception, the different gases operated with have, under equal volumes, specific heats proportional to their specific gravities; and of course that, under equal weights, they have the same specific heat. In the excepted case, that of hydrogen, the specific heat is nearly the double of that which would result from this law.

### *On the absence of Magnetism in Cast Iron when in fusion.* By R. W. Fox.

In the course of some magnetic experiments, it appeared to the author desirable to ascertain whether a magnetic needle is acted upon by cast iron in a state of fusion. For this purpose he had a horizontal mould made in sand, about five feet long and two inches square, in the direction of the magnetic meridian; and at a very small distance from its northern extremity, and parallel to it, he placed the south pole of a delicately poised magnetic needle, the north pole of which extended beyond the mould. The latter was then filled with very fluid melted iron, but not the slightest effect was produced on the needle till after the metal had become fixed and cooled down to a cherry-red colour. The needle was then very suddenly attracted with great energy. Sand and a copper plate were employed to protect it from the hot iron.

This experiment may perhaps be considered by those who advocate the existence of a high temperature in the interior of the earth, as tending to strengthen the arguments in favour of the agency of electricity in producing terrestrial magnetism, seeing that intense heat and fixed magnetism, in the ordinary acceptation of the term, cannot, apparently, exist together.*

### *On Electric Currents passing through Platinum Wire.* By William Barker, M.D.

When the large deflagrating battery was used on two occasions in the Chemical School of Trinity College, Dr. Barker observed that on passing the current of electricity through a piece of platina wire, about three feet in length, and igniting it, there were dark portions of the wire of about $\frac{1}{4}$ or $\frac{1}{2}$ an inch in length at intervals of from three to four inches in its whole length, the same parts of the wire being in the same condition during the time that the wire remained ignited. The author was unable at the time, owing to the number of experiments to be tried with the battery, to take any measurements or to examine whether wires of different diameters were differently affected. The fact was stated for the consideration of the Section, reserving for further examination the law by which the distances and dimensions of these unignited portions of the electrified wire are governed, whether their distances are constant, or vary according to the size and material of the wire employed or the quantity or intensity of the galvanic currents.

* [Mr. Peter Barlow had shown, many years since, that all magnetic action was lost by iron when raised to a white heat. See Phil. Trans. 1822, or Phil. Mag., first series, vol. lx. p. 345.]

*An Account of some Experiments recently made on the Buoys in Kingstown Harbour, with a view to protect from the action of Sea Water the Metals, and especially the Iron-work, attached to them. By* EDMUND DAVY, *F.R.S., M.R.I.A., &c., Professor of Chemistry to the Royal Dublin Society.*

Last year an enlightened member of the Royal Dublin Society, Mr. John M'Mahon, made the author acquainted with the fact that the iron-work attached to the new buoys lately put down in Kingstown Harbour had undergone a very rapid corrosion by the action of sea-water on it; and shortly after, the Commissioners of Public Works acting as Commissioners of Kingstown Harbour directed his attention to the subject, with the view of ascertaining the cause of such corrosion, and the means of prevention.

The new buoys* are precisely similar to the buoys, of the most approved construction, now used in Portsmouth Harbour. The whole surface of each buoy is sheathed with copper, except the bottom and about three inches of the smaller end, which is covered with lead, fastened to the copper by metal nails. A bolt passes through the whole length of the buoy, and is terminated at each end by a shackle. The lower shackle has a bridle patent chain fastened to it by means of a bolt and a thin pin called a *forelock*, which is such an important part that on its preservation mainly depends the security of the ships moored to the buoy†. The bridle chain is secured to a larger chain-cable and moorings, by means of shackles, bolts, and forelocks. The forelocks require to be examined about once a year, and replaced if defective. The bolt, shackles, chains, and forelocks are all of the best wrought iron.

On examining the buoys the author found all the iron-work at and near their bottoms very much corroded; and the corrosion appeared to be most considerable on the iron in the immediate vicinity of the lead, where it was about one eighth of an inch deep, and the metal was so much indented as to exhibit a coarse fibrous structure. So rapidly had the iron-work corroded in about six months, that had it continued at the same rate for two years the buoys (in the opinion of competent judges) would have been quite unfit for the public service. The copper and lead attached to the buoys were in a good state of preservation.

The extraordinary corrosion of the iron-work appeared to be due to an electrical action produced in sea water by the contact of the iron with the lead joined to the copper, on the buoys; these metals being preserved at the expense of the iron. The author submitted his views on the subject to the Commissioners, and suggested the propriety of removing a circle of about three or four inches of lead

* The author exhibited a drawing of one of these buoys.

† Some years since the Lord Lieutenant's yacht broke from her moorings in Kingstown Harbour in consequence of the defective state of the forelock.

from the iron-work at the bottom of each buoy, and of driving two or three short large-headed iron nails through the remaining lead into the wood, in order to protect both the lead and copper covering of the buoys from corrosion. These suggestions being promptly carried into effect, the author has during the last twelve months had frequent opportunities of examining the state of the iron-work attached to, and in the immediate vicinity of, the buoys, and he states that the removal of the lead has put a stop to the very rapid corrosion of the iron-work.

The action of sea-water on iron, under ordinary circumstances, is, as is well known, by no means inconsiderable. The author found that a piece of iron chain weighing 14 pounds 5 ounces, when exposed for 24 hours in $5\frac{1}{2}$ quarts of sea-water, lost 70 grains, and in a few days upwards of a quarter of an ounce: these facts led him to think it both desirable and practicable to coat the iron-work of the buoys, &c. with a varnish or japan which should be impervious to sea-water: and at the request of the Commissioners he made many experiments, using different varnishes and japans; but the results obtained were for the most part of a negative kind, owing not only to the action of sea-water on iron, but also to the constant friction to which the metal must be exposed, from the unceasing influences of tides, winds, and the strains from ships. He has hitherto found no varnish or japan that he can recommend as a means of preventing, for any length of time, the ordinary corrosion or oxidation of iron in sea water.

The author made a number of experiments with a view to apply metallic protectors to the iron-work connected with the buoys, on the principle developed by the late Sir H. Davy. He found that when small ingots of zinc were attached to pieces of chain cable in sea-water, during several weeks, these lost no weight, and the corrosion of the zinc was inconsiderable. Hence it seemed obvious, that zinc will protect iron from corrosion in sea-water. These results were so satisfactory that the author recommended the experiments to be tried on the buoys, and the Commissioners immediately requested him to carry the same into effect. He has had under a course of trial for several months, in contact with the iron-work at the bottom of each buoy, two zinc protectors, each of which is about 6 inches long and $\frac{3}{4}$ inch wide, and weighs about 8 ounces; and on a recent examination, the iron-work near the zinc exhibited a clean appearance. There is another and a still more recent application of the zinc, which the author thinks will be very beneficial in protecting a most important part of the iron-work already alluded to, namely, the *forelock*. Several of the forelocks have stout zinc rings cast into holes made in their heads, and on lately examining a forelock so protected for several weeks, it was found quite free from corrosion.

The late Sir H. Davy referred the corrosion of copper in sea-water to the agency of the oxygen of the air. The author from his experiments has obtained results which lead to the same conclusion

with regard to iron. He found also that the corrosion of iron in sea-water is materially influenced by the depth of water in which the metal is immersed. He is of opinion that the wear of iron-work exposed to sea-water is more considerable the nearer the iron is to the surface or to the external air. The principal wear of the iron-work connected with the buoys seems to be at and within a few feet of the surface of the water; and this portion of the iron may be protected by attaching strong pieces of zinc to it.

The corrosion of iron in sea-water, under ordinary circumstances, appears to arise from exposure of the water to the atmosphere, and the consequent gradual absorption of its oxygenous part. The protection of iron in sea-water by the contact of zinc seems due to a simple electrical action between the respective metals and the fluid; water being decomposed, its hydrogen is evolved, its oxygen goes to the zinc, whilst the oxide of zinc as it forms seems to be deposited on the iron, at least in part.

The author made a number of experiments to ascertain whether zinc would protect iron in sea-water if a very thin surface of glass, wood, paper, tow, &c. were severally interposed between those metals, but the results seemed clearly to prove that actual contact of the metals is indispensably necessary to that effect.

Zinc will protect iron in fresh water. The author has made experiments on this subject, and has others still in progress; the results of which may admit of useful applications to valuable parts of machinery, &c.

The author expressed his obligations to Mr. Hutcheson, the Harbour-Master at Kingstown, for the kind and prompt assistance he afforded on every occasion, and for the interest he took in the progress of the experiments on the buoys, &c.

*On some recent Experiments made with a view to protect Tin Plate or tinned Iron from corrosion in Sea-water, with some probable applications; and on the Power of Zinc to protect other Metals from corrosion in the Atmosphere. By* EDMUND DAVY, *F.R.S., M.R.I.A., &c., Professor of Chemistry to the Royal Dublin Society.*

If a piece of tin plate is exposed in sea-water for a few days, it will exhibit an incipient oxidation, which will gradually increase; the tin will be preserved at the expense of the iron, which will be corroded. But if a small surface of zinc is attached to a piece of tin plate and immersed in sea-water, both the tin and iron will be preserved, whilst the zinc will be oxidated,* on the principle first made known by the late Sir H. Davy.

The author has exposed for nearly eight months in sea-water a surface of tin plate nailed to a piece of wood by means of tinned iron tacks, inserting between the wood and the tin plate a small button of zinc. Under these circumstances the tin plate has remained

clean and free from corrosion; the zinc has of course been corroded. In a comparative experiment, in which a similar piece of tin plate was nailed to the same piece of wood, and exposed during the same period to the same quantity of sea-water, without the zinc, the edges on two sides of the tin plate were quite soft from the corrosion, which had extended to about $\frac{1}{8}$th of an inch. These experiments seem worthy of being repeated and extended.

The present demand for tin plate is very great; should these statements be confirmed, a vast increase in its consumption might be anticipated. The opinion may be entertained that it is practicable to substitute double tin plate for sheet copper in covering the bottoms of ships, &c., using zinc in small proportion as a protector. Such applications would probably occasion a saving of nearly three fourths of the present expense of copper sheathing.

It also seems deserving of inquiry whether tin plate vessels, protected by zinc, may not be advantageously substituted for copper vessels in many of our arts and manufactures, and even in domestic œconomy. Although it might be presumed from Sir H. Davy's experiments and observations* that zinc would protect tin plate from corrosion in sea-water, the author is not aware that any direct experiments on the subject have been published. Sir H. Davy briefly refers to some obvious practical applications of his researches to the preservation of finely divided astronomical instruments of steel by iron or zinc; and that Mr. Pepys had taken advantage of this last circumstance in inclosing fine cutting-instruments in handles or cases lined with zinc. The author has not heard whether such applications have succeeded, but he has made a number of experiments with a view to protect brass, iron, copper, &c. from tarnish and corrosion in the atmosphere by means of zinc; the results obtained, however, lead to the conclusion that contact with zinc will not protect those metals in the atmosphere, the electricity thus produced, without the intervention of a fluid, being apparently too feeble to counteract the chemical action of air and moisture on the surfaces of these metals.

*On the comparative value of Irish and Virginian Tobacco. By* EDMUND DAVY, *F.R.S., M.R.I.A. &c., Professor of Chemistry to the Royal Dublin Society.*

In the year 1829–30 the cultivation of tobacco in Ireland excited much attention among agriculturists, and several hundred acres of it were raised in different counties; in consequence, the attention of the Royal Dublin Society was directed to the subject, and the author was requested by a select committee of that body to institute experiments on tobacco with a view to determine some questions of a

* Phil. Trans. vol. cxiv. for 1824; or Phil. Mag., first series, vol. lxiv. pp. 30, 233; vol. lxv. 203.

practical nature, as whether its root contained nicotin, and in what quantity, and to ascertain the comparative value of Irish and Virginian tobacco.

The author's experiments were made on average samples of Virginian and Irish tobacco; for the former he was indebted to the kindness of Mr. Simon Foot, and for the latter to Messrs. Wild, Cuthbert, Callwell, and Brodigan. From a number of experiments the author was led to conclude that the dried roots of Irish tobacco contain from four to five parts of nicotin in one hundred parts; and that one pound of good Virginian tobacco is equivalent in value to about $2\frac{1}{3}$ pounds of good Irish tobacco.

After the author had finished his experiments it was gratifying to him to be informed that some manufacturers estimate one pound of Virginian tobacco as equivalent in value to about two pounds of Irish. Hence there seems to be a pretty near coincidence between their results and those derived from a chemical examination.

*On Nicotin and some of its Combinations.* *By* EDMUND DAVY, *F.R.S., M.R.I.A., &c., Professor of Chemistry to the Royal Dublin Society.*

When the author commenced his experiments in 1829 on Irish and Virginian tobacco, nearly all our knowledge of the peculiar principle in tobacco, called *Nicotin* by the late M. Vauquelin, was confined to his paper on tobacco*. By a series of processes in which the expressed juice of tobacco was reduced to one fourth of its bulk by evaporation, then digested in alcohol, distilled, again concentrated, dissolved in alcohol, then evaporated to dryness, dissolved in water, saturated with potash, and distilled to dryness, Vauquelin seems to have obtained a fluid nearly approximating to the nicotin recently procured.

In obtaining nicotin, the author avoided the circuitous processes of Vauquelin, and adopted only the simple method of exposing tobacco to the action of a solution of potash and subsequent distillation. The alkali employed was in some cases weak and in others *strong. In some instances it was macerated on the tobacco for one* or two days; in others, it was added to the tobacco in the retort and distilled at once. Other fixed alkaline substances in solution, as soda, barytes, strontites, lime, may be substituted for potash. Distillation was occasionally carried on below, but in general at the boiling-point. Under such varied circumstances, the fluid procured, on being rectified by a second distillation, is an aqueous solution of nicotin, having the following properties. It is colourless and transparent. Its odour closely resembles that of tobacco, but is far more pungent. Its taste is peculiar, and leaves a sharp biting impression on the tongue for some time. It changes turmeric paper to brown;

* *Annales de Chimie*, tome lxxi.

but this effect is not permanent, but gradually disappears on exposure to the air. Its specific gravity (according to repeated trials made by two intelligent pupils of the author, Mr. Richard Austin and Mr. John Keogh, who assisted him in many of his experiments,) is about that of distilled water. It neutralizes the mineral and vegetable acids, forming peculiar salts, some of which the author has obtained in a crystallized, and others in an imperfectly crystallized state. It undergoes no apparent change by being kept in close vessels for a considerable length of time. It is volatile below the point of boiling water. It precipitates the greater number of metals from their solutions, as those of silver, mercury, tin, antimony, manganese, of a white colour; iron of a green, cobalt of a pink, and gold and platina of a yellow colour.

*Salts of Nicotin.*—A number of the salts of nicotin, as the nitrate, sulphate, &c., crystallize in four- and six-sided prisms; they are characterized by having a sharp biting taste, analogous to that of aqueous nicotin: they are mostly soluble in water, and are easily decomposed by a slight increase of temperature. The nitrate is so susceptible of change, that it seems to undergo an incipient decomposition when exposed in solution for a few hours, and assumes a reddish colour. The author's experiments have led him to conclude that nicotin is composed of carbon, hydrogen, oxygen, and nitrogen, but he is not yet satisfied as to its exact constitution. He made some experiments to try the effects of aqueous nicotin on small fishes, flies, moths, spiders, &c. A few drops of it diffused in a tumbler of water strongly acted on the nervous system of small fishes, immediately communicating to them an unusual but momentary energy, which was speedily followed by torpor.

Butterflies, moths, spiders, were soon killed by being brought in contact with a weak solution of nicotin. Common flies resisted its action better than spiders, drones, bees, or wasps, and after immersion for a short time, again recovered on being exposed to the air for a few minutes. Common caterpillars of a large size, on being taken from cabbages, and instantly put into a weak solution of nicotin, exhibited some energy, but presently became insensible, and being considered as dead were suffered to remain in the solution for about half an hour; they were then removed to fresh water, but exhibited not the slightest symptoms of life, but on being placed on a grass-plot near the house they all recovered, and were very active in the course of an hour.

The author is of opinion that aqueous nicotin may admit of a number of useful applications, as in preparing specimens of natural history for the museum, in preventing the destructive effects of the insect tribes which infest plants and trees in gardens, conservatories, &c. And it seems highly probable that the salts of nicotin will admit of useful medicinal applications.

After the author had ascertained the principal facts already stated respecting nicotin, he found that he had been anticipated; he observed in the 'Quarterly Journal of Science, Literature, and Art,' for

December 1829, that MM. Posselt and Reimann had lately obtained a vegeto-alkali from tobacco, examined its properties, and combined it with a number of acids. But though those chemists are justly entitled to the merit of having first made known to the public an interesting series of facts respecting nicotin, the author's experiments may serve to corroborate their general results, and also throw additional light on the subject.

### *On a Fluid obtained in the manufacture of Pyroxylic Spirit. By* M. Scanlan.

The author has been for some time past engaged in the making of pyroxylic spirit, a fluid now extensively used in England (under the name of 'Naphtha') as a substitute for alcohol, principally by hat-manufacturers, for the purpose of dissolving shell lac and mastic to stiffen their hats and render them water-proof.

In the process which Mr. Scanlan pursues, he obtains a fluid of a higher specific gravity, but having a lower boiling-point than pyroxylic spirit, and differing from it in other respects.

Rough pyroligneous acid is submitted to distillation in a copper still, by the maker, in order to separate some of the tar it holds in solution; he sets apart the first 15 per cent. that distils over, and this he sells as wood-spirit. This liquor, as it comes from the pyroligneous acid-maker, contains much free acetic acid and tarry matter.

The author proceeds to saturate the acetic acid by means of slacked lime, which causes the separation of some pitch.

He next submits the saturated liquor to distillation as long as the distilled product is of less specific gravity than water.

This last product is rectified in a still somewhat on the plan of those for a long time in use on the Continent, and now coming into general use in this country, for the purpose of rectifying spirit. It consists of a boiler, containing the liquor submitted to distillation, and of a rectifier, which is a copper vessel of peculiar construction, placed in a bath of water, which must be kept at such a temperature as will condense water, but still retain the more volatile products in the state of vapour till they pass into the last part of the apparatus, where they are condensed and finally cooled.

In this process of rectifying, the author was a good deal surprised to find the product first condensed had a higher specific gravity than that which succeeded to it in the distillation. The first being about ·900, and the second as low as ·830; to this, if the distillation be pushed far enough, succeed water and an oil which becomes black by keeping. The fluid having specific gravity ·900, is a good deal coloured; treated with animal charcoal its colour is removed; rectified from a water-bath after treatment with animal charcoal, its specific gravity is ·911, and its boiling-point about 132°.

In this state it is colourless and inflammable: it has a powerful, and to most persons a very disagreeable smell. Caustic potash decomposes it instantly, acetate of potash being formed, and probably carbonate of potash. It forms acetate of lime also when slacked lime is added to it. It softens copal, but dissolves very little of it. When diluted with water it does not comport itself as alcohol of the same specific gravity does; 50 measures of it mixed with 50 of water at the temperature of 54, were raised in temperature to 61, and a considerable quantity of air was extricated; the mixture brought again to the temperature of 54, measured but 96·5 measures, and its specific gravity was ·9861. Alcohol diluted so as to have specific gravity ·911, when similarly treated, measured 98, and its specific gravity was ·9659.

Litmus-paper immersed in it is not reddened, but on exposure to the air the fluid evaporates and leaves the paper permanently red.

It mixes with water in every proportion, and water may be separated from it by means of carbonate of potash as from dilute alcohol, which is not the case with pyroxylic spirit.

*On the Chemical Constitution of Fossil Scales, as illustrative of the nature of the Animals from which they have been derived.* By Arthur Connell.

The difficulty of determining merely from external characters whether a fossil scale has belonged to a fish or to a saurian animal, and the geological interest which that problem frequently possesses, render it desirable to know whether chemical means are capable of solving it.

Mr. Hatchett ascertained that the scales of recent reptiles consist chiefly of a horny substance, whilst those of fish contain a considerable proportion of phosphate of lime, and are of the nature of bone. Chevreul confirmed his observation as to fish-scales; and the author has found that the scales of small recent crocodiles contained little more than one per cent. of incombustible earthy matter, although in the carinated dorsal scales the amount extended to about 3 per cent. When fish-scales are fossilized we may therefore expect that the bone-earth will remain, and the perishable animal substance will either disappear without any substitution, or be wholly or in part replaced by siliceous or calcareous matter; whilst, on the other hand, if a saurian scale is mineralized it ought to consist almost entirely of some replacing substance, such as siliceous or calcareous matter, coming in place of the decaying animal matter and of little or no bone-earth.

The author has analysed fossil scales from the three following localities, and the result of the analysis he conceives to show the whole of them to have belonged to fish:

| | Burdie-house. | Craighall Coal. | Tilgate. |
|---|---|---|---|
| Phosphate of lime | 50·94 | 55·75 | 60·13 |
| Carbonate of lime | 11·91 | 15·86 | 27·94 |
| Siliceous matter | 36·58 | 16·17 | 3·42 |
| Potash and soda | ·47 | 1·06 | 1·43 |
| Alumina | ...... | 2·82 | ·82 |
| Bituminous matter and water* | ·12 | 6·46 | 6·71 |
| Phosphate of magnesia | Trace. | | |
| Animal matter | Trace. | | |
| | 100·12 | 98·12 | 100·45 |

In the first of these the animal matter appears to have been replaced by siliceous matter; in the two others, partly by siliceous matter, and partly by carbonate of lime.

The author has had no opportunity of examining an undoubted saurian fossil scale.

*On the Composition and Properties of the Salts of Sulpho-Methylic Acid. By* ROBERT J. KANE, *M.D., M.R.I.A.*

Professor Kane had been occupied with experiments on pyroxylic spirit, in order to test the truth of Liebig's idea of its nature, and had announced to the Royal Irish Academy the fact of the formation of a peculiar acid, analogous to the sulphovinic, by the action of sulphuric acid, before he received an account of Dumas and Peligot's researches on that substance. The question of its nature having been decided by their analysis, he restricted himself subsequently to the development of the history of the sulpho-methylates, a department of the subject in which the French chemists had but slightly touched.

The sulpho-methylates are easily prepared. A salt of lead may be procured by mixing pyroxylic spirit with an equal weight of oil of vitriol, and neutralizing by carbonate of lead. It crystallizes in fine long rectangular prisms. A salt of baryta can be obtained in a similar manner with carbonate of baryta. From either of these salts the other sulpho-methylates can be obtained, by double decomposition, by means of a soluble sulphate.

The sulpho-methylate of potash crystallizes in pearly rhomboidal plates; it deliquesces. Heated it gives water, neutral sulphate of methylene, and sulphurous acid, leaving a carbonaceous residue of sulphate of potash. The mean of three analyses gave for its composition,

* The Tilgate scales contained carbon and sulphur instead of bituminous matter.

| | |
|---|---|
| Potash | 29·51 |
| Sulphuric acid | 50·10 |
| Methylic æther | 14·39 |
| Water of crystallization | 6·00 |
| | 100·00 |

which agrees with the formula:

$$\overline{2\,\dddot{S}+\dot{K}+(2\,C+3\,H+O)}+\dot{H}$$

The sulpho-methylate of baryta crystallizes in plates. The mean of two analyses gave

| | |
|---|---|
| Baryta | 38·50 |
| Sulphuric acid | 40·21 |
| Methylic æther | 11·49 |
| Water of crystallization | 9·80 |
| | 100·00 |

This salt was analysed by Dumas. His result:

| | |
|---|---|
| Baryta | 38·6 |
| Sulphuric acid | 40·4 |
| Methylic æther | 11·1 |
| Water of crystallization | 9·9 |
| | 100·0 |

Both analyses indicate the same formula:

$$\overline{2\,\dddot{S}+\dot{B}a+(2\,C+3\,H+O)}+2\,\dot{H}$$

The sulpho-methylate of lime crystallizes in octohedrons, which are anhydrous. They deliquesce, and by the mean of two analyses are composed of,

| | |
|---|---|
| Lime | 21·41 |
| Sulphuric acid | 60·25 |
| Methylic æther | 18·34 |
| | 100·00 |

giving the formula $2\,\dddot{S}+\dot{C}a+(2\,C+3\,H+O)$.

The sulpho-methylate of lead usually obtained is in long prisms, which readily deliquesce, and are very easily decomposed, being resolved into sulphate of methylene and sulphate of lead. The mean of seven analyses of this salt gives for its composition,

| | |
|---|---|
| Oxide of lead | 49·76 |
| Sulphuric acid | 35·93 |
| Methylic æther | 9·81 |
| Water of crystallization | 4·50 |
| | 100·00 |

giving the formula $2\,\dddot{S}+\dot{P}b+(2\,C+3\,H+O)+\dot{H}$.

On two occasions a lead salt was obtained in plates like the baryta salt, and apparently containing two atoms of water; but Professor Kane has not determined the exact circumstances necessary to the production of this form, and consequently its examination yet remains to be made.

The salts of copper, nickel, soda, ammonia, lime, magnesia, alumina, and iron have been formed by double decomposition, but their properties would occupy too much space in describing. The mode of obtaining them indicates their composition.

All those salts that contain crystallization water, lose it (efflorescing) when dried over sulphuric acid. This method was employed to determine the quantity of such water present.

Dr. Dalton observed that he had analysed pyroxylic spirit some years since (in 1829), and found it to be composed of an atom of olefiant gas united chemically to one of water. This was inferred from burning its vapours with oxygen in Volta's eudiometer. He also ascertained that burning it in a lamp produced the same heat as burning alcohol diluted so that the two liquids contained the same relative quantities of olefiant gas and water. At the same time he found pyroacetic spirit to be constituted of 3 atoms of carbon, 2 of hydrogen, and 1 of oxygen, or rather 1 atom carbonic oxide holding 2 of olefiant gas: this was discovered by burning the vapour with oxygen in Volta's eudiometer.

*On some Combinations of Protochloride of Platina with Protochloride of Tin. By* Robert J. Kane, *M.D., M.R.I.A.*

These bodies unite in two different proportions; that containing least tin is of an olive brown colour, crystalline, and very deliquescent; decomposed by much water, giving muriatic acid and mixed oxides of tin and platina. The second, which contains most tin, is of an intensely red colour, soluble in a small quantity of water, giving a splendid red solution, but is decomposed by much water, giving muriatic acid, and a chocolate powder which contains the protochlorides of platina and tin and protoxide of tin. By acting on this powder by ammonia, a black matter in crystalline grains is obtained, which when heated burns like tinder, with formation of peroxide of tin, and platina is reduced.

The colour of the solution was found by Professor Kane, on examination by a prism, to be an absolutely homogeneous red.

Professor Johnston read a paper on the physical cause of certain optical properties observed in chabasie.

The nature and amount of the double refraction are found to vary according to the course taken by the ray; and this Mr. Johnston conceives to arise from the fact of the index for quartz being nega-

tive, and that for chabasie positive, and from certain crystals of chabasie including an excess of silica, which is a substance plesiomorphous with chabasie. (Dr. Thomson stated that there are two distinct species of chabasie, one of which includes soda and the other lime, and from the admixture of which the phænomena might perhaps arise.)

Professor JOHNSTON stated verbally the results of his analysis of the single and double iodides of gold, results which he found to correspond generally with those already obtained for the chloride.

Professor GRAHAM gave an account of some recent researches which he has published in reference to the constitution of certain compounds as far as respects their constituent water. He illustrated his views by sulphuric acid, with 1 and 2 atoms of water, by oxalic acid with 1 and 3 atoms of water, and by nitric acid containing 1 and 4 atoms of water. Other compounds were also adduced, such as oxalate of magnesia, which contains two atoms of water, or that which may be considered as the water of crystallization of oxalic acid. The oxalate, binoxalate, and quadroxalate of potash, and several other saline compounds were also brought forward in explanation of his views.

Anhyd. oxal. a.. $(\ddot{C}+\dot{C})$

Oxal. water.... $(\ddot{C}+\dot{C})\dot{H}$

Oxal. acid..... $\dot{H}(\ddot{C}+\dot{C})2\dot{H}$

Oxal. potash... $\ddot{K}(\ddot{C}+\dot{C})\dot{H}$

Binox. pot..... $\dot{K}(\ddot{C}+\dot{C})\dot{H}+(\ddot{C}+\dot{C})2\dot{H}^2$

Quadrox. pot... $\dot{K}.(\ddot{C}+\dot{C})\dot{H}+(\ddot{C}+\dot{C})2\dot{H}+\dot{H}(\ddot{C}+\dot{C})\dot{H}^2$

He then drew attention to ammonia, which he considered as frequently performing the function of water in saline compounds; a view which he impressed upon the Section by drawing attention to the composition of the sulphate and of two distinct ammoniurets of copper.

*On a new Method of testing the presence of Muriatic Acid in Hydrocyanic Acid. By Professor* GEOGHEGAN.

This proceeding is essentially preliminary to the adoption of the usual modes of determining the strength of any given specimen of this agent. The insoluble compounds into which the chlorine of muriatic acid enters, and by the formation of which chemists usually recognise its presence, are known to resemble, in many respects,

those to which cyanogen gives rise when combining with the same bases. The method proposed by Dr. Geoghegan is founded on the property which the double salt of the iodide of potassium and bicyanide of mercury possesses of being decomposed by acids, and then producing biniodide of mercury. This compound, which has been analysed by Liebig, and subsequently by Dr. Apjohn, is easily prepared by mixing, in the proportion of atom and atom, the iodide of potassium and bicyanide of mercury, each dissolved in a small quantity of hot water. After a short time silvery scales (resembling acetate of mercury) are formed, which constitute the salt in question. The circumstance of this salt being decomposed by all the ordinary acids, would appear to show that it is not capable of demonstrating the presence of muriatic acid in particular; but as the only other impurities likely to be present in the hydrocyanic acid are sulphuric and tartaric acids, if the appropriate tests of these latter do not indicate their existence, then the formation of biniodide of mercury on the addition of a crystalline scale, or solution of the double-salt above mentioned, may be considered as furnishing conclusive evidence of the presence of muriatic acid. It may be also stated, that the only hydrocyanic acid likely to contain sulphuric—that prepared from the ferrocyanide of potassium—can be generally recognised, as to the source from whence derived, by its possessing a slight bluish or bluish-green tinge, which is quite distinctive. The mode of detecting the presence of muriatic acid above detailed has the advantage over those usually employed, of being very readily applied, and the formation of the reagent is perfectly simple; it is capable of detecting 1-4500th part of the acid: if no change of colour ensue on the addition of the salt, we may conclude that the specimen of hydrocyanic acid contains no impurity which could interfere with the subsequent estimation of its strength. This method, however, is inapplicable to the alcoholized acid of Germany, as the biniodide is soluble in spirit, yielding a colourless solution. If the presence of muriatic acid have been ascertained, its neutralization can be readily effected by the addition of successive small portions of precipitated carbonate of lime, as long as any is dissolved; when free, muriatic acid has been got rid of, and not till then can the estimate of the strength of the specimen under examination be proceeded in with any hope of a correct result. The method of Dr. Ure for effecting this latter end is sufficiently correct for ordinary purposes, if we substitute for the red precipitate which he employs, pure peroxide of mercury; as, independent of the presence of minium and other impurities, red precipitate is seldom, if ever, free from pernitrate of mercury: if perfect accuracy be desirable, the best method, and probably as simple a one as that just alluded to, is the formation of cyanide of silver by the addition of the nitrate of that metal.

### *On Bleaching certain Varieties of Turf for the Purpose of producing a White Fibre for the manufacture of Paper. By* R. Mallet.

The kind of peat used for this purpose is that which exists immediately beneath the vegetable surface of almost every lowland or flat bog in Ireland, and is found existing in a stratum frequently of about three feet thick. It consists of the leaves and stems of various mosses, the roots and fibres of many small aquatic and marsh plants, &c. in the first stage of that very slow decomposition which is the character of every peat moss.

The fibres are tough, and retain perfectly, in most instances, their original form, and are arranged more or less in parallel strata; its colour is a reddish brown, and its specific gravity, as obtained from various bogs, varies from ·360 to ·650. It is proposed either to use the fibre bleached from this for paper-making alone, or in place of the various adulterations now used in paper from rags, such as chalk, gypsum, clay, cotton-*flyings*, hair, leather-cuttings, hop-bines, &c.

The same material is capable without bleaching of being converted into an excellent species of board paper or mill-board, by simple pressure under an hydraulic or other press, and subsequent saturation in an exhausted vessel, with glue and molasses, drying oil, rosin, and oil, or any other suitable material. When so treated, it will withstand well the action of high-pressure steam.

This species of turf contains from 3 to 11 per cent. of ashes when humid, and when dried, merely atmospherically, from 4 to 6 per cent. of water. The ashes are of a white or yellowish white colour, and contain,

| | | |
|---|---|---|
| Carbonate of lime | 69·5 | |
| Silica | 3·0 | |
| Alumina | 17·0 | |
| Peroxide of iron | 8·0 | |
| Loss | 2·5 | 100. |

The author cannot account for the [illegible]s on this analysis, and has been unable to repeat it. He states that ashes from the bottom of the same bog where this red turf was obtained give a totally different result, viz.

| | | |
|---|---|---|
| Carbonate of lime | 21· | |
| Sulphate of lime | 5·5 | |
| Silica | 24·5 | |
| Alumina | 26·3 | |
| Oxide of iron | 22·0 | |
| Loss | 0·7 | 100. |

The fibrous matter of this red turf is intimately combined with various complicated vegetable results of slow decomposition, but containing in greatest proportion the extractive matter to which Berzelius has given the name Geine, *from* γῆ, *terra*. The extract obtained from turf in the way about to be described seems to be

nearly the same as that which he describes, in fact to be ulmin in an impure state.

The specimen of turf to be bleached for paper is softened in cold water until its parts by agitation will separate; the finer particles are washed off; the fibre which remains is digested in the cold with a very dilute solution of caustic potass or soda, containing only 50 grains of alkali to a quart of water. The solution, containing the geine in solution, is pressed from the fibres; the latter are then soaked for some time in very dilute sulphuric acid, consisting of 150 grains of the sulphuric acid of commerce, in a quart of water. The iron is obtained in solution, and the ammonia if any exist in the turf. The fibre is now again separated by pressure from the dilute acid, and digested in the cold, with dilute solution of chloride of lime, of the strength commonly used by paper-makers to bleach fine rags. After the bleaching has taken place the fibre is strained from the liquor, well washed, and applied to the manufacturer's purposes.

The extremely dark-coloured solution obtained by the caustic alkali is now treated with an excess of dilute sulphuric acid, and the acid of the previous washings may be in part used by the manufacturer for this purpose. The alkali is neutralized, and the geine precipitates. It is collected on a filter or by other suitable means, and well washed with cold water, and finally dried by a steam bath, after which, if perfectly dried, it ceases to be soluble in water. It may now be used either in oils or distemper as a colour, being a rich brown bistre.

The solution from which it has been separated contains sulphate of potass, and occasionally, in very minute quantity, sulphate of ammonia.

The quantity of soluble matter in the turf operated on was found from 14 to 30 per cent.; and from one hundred weight of turf of proper quality may be obtained about 18 pounds of fine white fibre fit for paper-making, and a much larger proportion of a coarser and less white description.

When the turf is digested in the chloride of lime, a thin film of an unctuous-looking matter floats after some time on the solution, and by careful management may be obtained in small quantity; it appears to be a mixture of a gum resin with something analogous to wax, and of artificial camphor.

This substance smells like common camphor. Its specific gravity is 0·990, which is a little more than that of camphor. It is at ordinary temperatures always partly solid and partly fluid. When deprived of adhering water it shows a tendency to crystallize; the more fluid part gradually evaporates when it is exposed to air, and a varnish is left on the vessel which contained it. Its point of homogeneous fusion is somewhere between 290 and 300; it evaporates rapidly between that and its boiling-point, which seems to be about 360. As it boils away, its boiling-point rises; it is insoluble in water; a great part dissolves in alcohol, and the remainder is soluble in caustic potass and in fixed oils.

Proof spirit dissolves from it a very minute quantity of a substance which seems to be a gum resin. It is entirely decomposed by a red heat, in close vessels, and also by concentrated and boiling sulphuric acid, which reduces it to charcoal, and a substance apparently analogous to artificial tannin.

The bistre, or colouring-matter, obtained from the turf is not affected by carbonic acid, nor by sulphuretted hydrogen, nor by protochloride of tin: strong nitric acid will not change its colour, although by long standing it is decomposed by it. Chlorine bleaches it slowly; caustic alkalies redissolve it. It is scarcely bleached at all by the sun's rays, nor does it when properly washed and dried show any tendency to deliquesce; it is therefore an excellent colour for paper-staining and other such purposes, as few common agents will injure it, and it can be readily removed from surfaces by an alkali.

The proportions of useful products above given can only be considered as approximations, having been deduced from experiments on a small scale; they would probably be much increased, and the relative expense of preparing the material reduced, if the process were carried on with greater quantities.

## *On some singular Phænomena of Flame from Coal-Gas.* *By* R. Mallet.

If an Argand gas-burner be lighted, and a conical tube of a certain diameter be inserted concentrically within it, with its extremity entering a certain distance, within the burner, and, while the gas is inflamed, a current of air be propelled through the conical tube in the same direction with the streams of gas, under certain conditions, the whole of the gas-flame will retract or be drawn back between the internal surface of the burner and the external surface of the conical tube, and nothing whatever will pass forward but a stream of strongly heated carbonic acid and aqueous vapour. This very singular phænomenon of the passage in opposite directions of two currents in such close contact does not appear to be affected by the size of the burner, provided a certain proportion be preserved between it and the conical air-tube. The experiments were made with two burners chiefly, one of which was three quarters of an inch internal diameter and one inch and a half deep, measured along its axis, and the other seven sixteenths of an inch internal diameter, and one inch and three eighths deep.

With these it was found that the retraction of the flame was produced most perfectly in the case of the large burner by a tube of five sixteenths of an inch diameter, but yet took place to a certain extent until the diameter of the tube was reduced to one eighth of an inch, and in the case of the smaller burner it was most perfectly produced by an air-tube of three sixteenths of an inch diameter;

yet taking place in a slight degree with one of only one twentieth of an inch diameter.

If the conical air-tube be not inserted into the burner, but merely held close to its base or lower aperture, no retraction takes place, the flame is merely curtailed, and the combustion rendered more perfect; and the same result takes place when a tube equal in diameter to the internal part of the burner is used, in which case it is obvious none of the flame could retract.

To the perfect production of the foregoing effects it is necessary that the apertures for the gas in the burners be of a much smaller size and more numerous than usual. When the axis of the conical air-tube is parallel with that of the burner, the direction of each separate jet of flame from the holes in the burner is also parallel to the same while the air-tube and burner are respectively concentric; but if, while they remain concentric, the axis of the air-tube be inclined to that of the burner, a far more singular effect ensues: each separate jet of flame now in retracting describes a spiral round the internal surface of the burner, making from one third to perhaps one half a revolution.

If the conical air-tube, while still inclined as above, be now brought into contact with that side of the burner towards which it is inclined, the obliquity of the spiral is much lessened; but the flame is so much retracted at the side of the burner opposite the air-tube that it makes its appearance out at the lower end of the burner. The same effects are produced whether the burners are vertically, up or down, or horizontal, or inclined at various angles, subject to merely the disturbances produced by the ascent of the neighbouring currents of heated air.

The effects do not seem to depend upon difference of temperature between the current of air and the flame, as no change is produced by heating the former to upwards of 600° Fahrenheit, neither does the angle of the cone seem to be very essential, except it be so great as to nearly stop the aperture of the burner. A cylindrical tube answers equally well with a cone, but an inverted cone, that is, a tube terminating with an enlargement, will not produce the effects. Tubes of various other forms produce corresponding variations of the principal phænomena. A large flat disc, with an aperture just large enough to admit the burner, placed close to its perforated extremity, so as to prevent the passage of external currents parallel to the internal current of air, does not change the effects.

The retraction is considerably lessened, however, by stopping up the space at the lower end of the burner, between it and the air-tube, but is not wholly destroyed.

Another singular fact connected with these, remains to be mentioned: if a glass or copper tube, of about three eighths of an inch greater diameter than that of the burner externally, be placed over it, the same sonorous effect is produced as in the well-known experiment of the combustion of pure hydrogen, but much louder; indeed, the copper tube used, which was eighteen inches long and

one and three eighths inches diameter, emitted a most overpowering sound. Length of tube produced no variation in the state of the flame, nor did increase of diameter over the above limits, although both produced of course a change of musical note; but if the diameter of the tube, whether of glass or copper, was reduced to very nearly that of the external diameter of the burner, on approaching the end of the tube with the burner, the retractile flame was drawn forward, and, unless skilfully managed, was drawn out or extinguished at the moment the burner entered the tube; if, however, the introduction was successfully effected, the moment the burner came within the tube the flame again retracted as before. The sound ceased at the moment that the flame was extinguished.

The pressure of gas used in most of the experiments was that of the ordinary main-pipes in this city, about $1\frac{3}{4}$ inch of water; that of the current of air, which was produced by a good pair of double bellows, was equal to the pressure of a column of ($2\frac{1}{2}$ inches of) mercury; but it was found that no material alteration of effect took place from condensing the gas to about two atmospheres, and causing it to issue inflamed at that pressure, provided the pressure of the current of air was likewise increased in the same ratio nearly.

With a less powerful stream of air than was above stated, the effects were imperfectly produced; and with a much more powerful one the flame was blown out.

The temperature of the current of air heated by the flame, when it retracted best, was found, at the distance of four inches from the burner, to be 432° Fahrenheit, or perhaps a little higher. The combustion of the flame in all the foregoing cases is absolutely perfect; its colour is a deep blue, and the volume of intensely heated air propelled is very great, so that it may be rendered very useful for various purposes in the laboratory. It is not perfectly dry, but it is free from dust or smoke.

*On the Volatilization of Magnesia by Heat. By Professor* DAUBENY.

According to Von Buch, carbonate of magnesia must have been sublimed by volcanic action, although such a phænomenon would, Dr. Daubeny conceived, be scarcely admitted by chemists as consistent with the known properties of that earth.

A curious fact, however, confirmatory of the truth of Von Buch's opinion, occurred to Professor Daubeny in Italy. He visited a locality where there was an upper stratum of lava containing cavities. In one of these an English gentleman, resident on the spot, discovered a large quantity of carbonate of magnesia, and Professor Daubeny himself observed a minute portion of the same earth coating the outer surface of the lava. Here it is difficult to understand in what manner this substance could collect in the cavities or upon the surface of the rock, unless it had previously become volatilized by heat.

(Dr. Dalton observed that there could be no doubt that carbonate of magnesia might be volatilized, since Dr. Henry had informed him that a quantity of this substance was always driven off whenever the heat was carried beyond a certain point.)

Mr. Hartop made a communication on the use of the hot air blast in the manufacture of pig iron, in which he showed that the saving said to be effected by the use of hot air had been overrated, as a considerable portion of the alleged saving had been previously effected by other improved processes.

The general saving on the average he stated to be no more than 10*s.* per ton, and observed, that the price of *such* iron in the market had actually fallen from 15*s.* to 20*s.* per ton, while that from cold air at the same time rose 5*s.* per ton in Yorkshire. (This statement gave rise to observations on the part of several gentlemen, who stated that no such reduction in price of iron made by hot air had occurred in other parts of the country, and that, as prepared in Glasgow and many other places, it had not been deteriorated. This method has in consequence been adopted in every smelting-house in Scotland, and the annual produce of the works in that country during the last ten years has been nearly doubled.

Reference was also made to processes adopted in the Russian smelting-works, which showed that by a judicious adjustment of the quantities of cold air introduced by the blast, a saving could be effected approaching even to that obtained by the use of hot air.)

*Account of some Chemical Processes. By* Francis Barker, *M.D., Prof. Chem. Trin. Coll. Dublin.*

It has been known since the time of Bergman, that diluted acetic acid has little or no action on peroxide of iron; but it is not, perhaps, generally known that this oxide may be completely separated from sulphuric or muriatic acid, and probably from most other acids, by an alkaline acetate, the alkali exerting its usual action of detaching the peroxide, whilst the acetic acid remains inactive and does not unite with it, and that by means of the acetate of potash, peroxide of iron may be completely detached from the oxide of manganese, one portion of the acetate of potash decompounded by the salt of manganese producing acetate of manganese, which remains in a state of solution, whilst the other portion of the acetate of potash separates the peroxide of iron, on which the diluted acetic acid has no action. The advantages arising from this mode of operating are obvious, as it gives the chemical analyst the means of separating the oxides of iron and manganese by agents easily obtained and in the hands of every chemist.

As the success of this method depends in a great measure on attention to minute details in the mode of conducting the process,

more especially on the comparative quantities of the substances employed, a few experiments are adduced.

*Experiment.*—Five grains of green sulphate of iron taken and dissolved in fifty measured grains of cold distilled water: to this added, from the end of a dropping-tube, six drops of diluted nitric acid, spec. grav. 1·280. On applying heat to this mixture, it acquires a dark olive colour, arising from decomposition of the nitric acid by the protoxide of iron and absorption of the nitric oxide by the ferruginous solution. When the mixture is heated to ebullition this colour disappears, and is succeeded by the ordinary yellow colour of a solution of peroxide of iron. To the solution of the sulphate of iron, thus altered by the action of nitric acid, an aqueous solution of acetate of potash, containing one tenth of its weight of the acetate, is to be added, in the quantity of two hundred grains measured. The mixture, on this addition being made, changes to a dark reddish brown colour, nearly as intense as that of port wine. The mixture is now to be diluted with its own volume of water, and heat applied until it boils; the ebullition continued for about two minutes. The peroxide of iron begins to separate as the heat approaches the boiling-point, and in a short time the whole peroxide is detached. On filtering the mixture whilst hot, the fluid which passes through the filter appears colourless, and on addition of the the triple prussiate of potash, affords neither precipitate nor blue tinge indicating the presence of iron. The powder remaining on the filter, well washed with hot water, is of a clove brown colour.

The addition of the nitric acid with subsequent ebullition is essentially requisite to the success of this experiment; for if the green sulphate of iron be employed without the addition of nitric acid, on adding the solution of acetate of potash, and causing the mixture to boil, no change of colour to reddish brown is found to take place, but a black powder separates, and the mixture when filtered affords a fluid of a strongly ferruginous taste, yielding an abundant precipitate, of a bluish white colour, with the triple prussiate of potash; thus proving that the conversion of the oxide of iron into peroxide must precede the action of the acetate of potash, which is otherwise incapable of separating the oxide of iron from the acid.

When a solution of the green muriate of iron is treated in a manner similar to that above described, by converting the protoxide of iron into peroxide by nitric acid, and decompounding the solution by acetate of potash and heat, the same effects are produced as in the green sulphate of iron.

If to a solution of peroxide of iron, produced by the method above described, a solution of the oxide of manganese is added, then solution of acetate of potash and heat applied, a similar deposition of peroxide of iron takes place; and the filtered liquor, on addition of triple prussiate of potash, affords a cream-coloured deposit unmixed with any blue tinge: the peroxide of iron has therefore remained on the filter, and the oxide of manganese in solution

has passed through, yielding its proper precipitate with the triple prussiate. It is right to observe, that a solution of the muriate of manganese is not rendered turbid by admixture with acetate of potash and subsequent application of heat.

It follows from the preceding experiments, which have been many times repeated, that peroxide of iron may be completely separated from either sulphuric or muriatic acid by acetate of potash, and that in a mixed solution of peroxide of iron and oxide of manganese in an acid, a complete separation of the peroxide of iron may be effected by means of the acetate, provided that proper attention has been given to the comparative quantities of the ingredients employed in the mixture. Acetate of soda or of ammonia may be substituted for acetate of potash in producing this decomposition.

Two other chemical facts were adduced.

1. As the precipitation of the ammoniacal phosphate of magnesia is accelerated and made manifest by drawing lines with a blunt glass rod on the internal sides of the glass vessel in which the proper mixture is made for producing the precipitate, a fact first noticed by the late Dr. Wollaston, so in a similar manner the separation of bitartrate of potash from any mixture containing potash, to which tartaric acid has been added in proper quantity, will be accelerated and rendered manifest by drawing lines with pressure on the internal sides of the vessel with a glass rod, the crystals of bitartrate first attaching themselves to these lines.

2. That nitrate of lead like the nitrate of baryta is precipitated from water by addition of strong nitric acid, which in each case exerts a similar action, namely, that of abstracting the water from the salt.

*On a Source of Inaccuracy in Observations of the Dew-point. By the Rev.* WM. VERNON HARCOURT, *F.R.S.*

Mr. Harcourt having observed an apparent variableness in the deposition of dew on different surfaces, at the same temperature and in the same atmosphere, was induced to make the following experiments.

A pane of glass was rubbed, on different portions of its surface, with substances of different degrees of hardness, and left till the equality of temperature was restored: being then breathed upon, it was observed to show the condensed vapour in proportion to the polishing power of the substance by which the different parts of the glass had been rubbed; characters traced by a leaden point displayed this phænomenon in the greatest perfection. The experiment was next tried on metallic surfaces, by polishing, for instance, part of the blade of a rough razor, and breathing on it, when the same effect was obvious.

When the state of the dew on the different surfaces was examined with a lens, it appeared that its greater visibility on the more polished parts was owing to a stronger reflection of light from a greater number of minute and unconnected drops deposited on those parts.

It would seem as if the process of polishing insulates the points to which the particles of vapour attach themselves, and prevents them from running into each other; but though the vapour condensed on the polished surface thus becomes more sensible, it is not increased in quantity, as is easily proved by continuing to breathe on the pane of a window till streams of water run down on the unpolished surface, and on that only; and it is not a little remarkable, if the polish be also carried horizontally along the lower part of the pane, to observe the streams dammed up where they meet the polished part, and drops of water left along that portion of the line.

The observation of these facts led the author to apply some practical corrections to the ordinary method of ascertaining the dew-point: he adopts the direct process of Dr. Dalton, reducing the scale of the operation, and substituting metal for glass. A highly polished metallic vessel, not more than $\frac{3}{4}$ths of an inch wide and $1\frac{3}{4}$ inch long, is nearly filled with water; some crushed sal ammoniac is introduced; the salt is stirred up and mixed with the water by the bulb of a small thermometer, which falls in consequence very gradually, and when the dew appears the thermometer is in contact with the surface on which it is deposited. If a considerable depression of temperature is required, the vessel may be cooled down previously to the experiment by a similar process. This instrument, from the small quantity and cheapness of the cooling material, may be used constantly at little cost, and from the conducting and radiating properties of the vessel, as well as the precision with which it indicates the first deposition of dew, may probably be found to be uniform in its results.

Mr. Moore exhibited a leaden pipe which had served for about twenty years as the worm of a still for the distillation of medicated waters and spirits; at length it began to leak, and on examination it was found to be supported at various points by bars of wood crossing it, and to be tied at others with twine. Wherever it thus came in contact with either wood or twine, it was deeply corroded, and the lead appeared to be converted into a dark powder, which, when examined, was found to contain oxide and chloride of lead; at all other points the pipe was perfectly sound. The appearance of the corroded parts did not admit of the effect being attributed to mechanical action. The presence of chloride and oxide in the powder, established, he thought, that the corrosion was not entirely, if at all, caused by acids formed by the decaying organic matter; it appeared to him that it ought rather to be attributed to galvanic action, developed by the contact of the metal, and wood, or twine, which cause, acting for such a length of time, might be sufficient to accomplish the destruction of the pipe at the points of contact.

Mr. Ettrick, referring most of the unfortunate explosions which have happened in collieries where the "Davy lamp" is used, to the

ignorance and wilfulness of the workmen, proposed a plan for the security of the lamp from injury and mismanagement.

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Professor GRAHAM noticed with regard to safety lamps, on the theory of which he has been some time engaged, that wire gauze is rendered much more impervious to flame by being first dipped in an alkaline solution, which also protects the wire from oxidation.

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*On a new Electrometer. By* WILLIAM SNOW HARRIS, *F.R.S., &c.*

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*Report of the Committee appointed to consider the subject of Chemical Symbols. By Dr.* TURNER.

[This Report, with the remarks of several of the members of the Committee, will appear in the next volume of the Transactions of the Association.]

## GEOLOGY AND GEOGRAPHY.

*On the Geological Map of Ireland. By* R. J. GRIFFITH.

Mr. Griffith presented his Geological Map of Ireland, the result of many years' research and labour, assisted in part by the publications of Weaver, Conybeare, Buckland, and Berger. Mr. Griffith, in pointing out the inaccuracies of existing maps of Ireland, dwelt on the advantages which will be derived from the publication of the Ordnance maps of Ireland, four counties of which have now appeared. At present great difficulties attend the allocation of geological phænomena, which are frequently misplaced in relation to each other, from the necessity of following the defects of the old maps. Mr. Griffith, as an example, stated that in Arrowsmith's map, Benwee Head is placed twenty miles north of the parallel of Sligo, though it is actually due west of that town. The remarkable position of the mountain masses was first pointed out.. They occur on the margin of the island, and enclose the great central limestone plain; an arrangement which shortens the courses of the rivers, rising as they do in the higher grounds, and rapidly descending to the sea. The Shannon is an exception, having a course of 140 miles; but it also is affected by the peculiarity alluded to, its stream falling eighty feet in the first twenty miles of its course, and only eighty feet more in the remaining 120. On the great plain which occupies the centre of the island numerous beds of gravel occur, called Escars, which though constant in direction when considered in reference to small spaces, are variable when the comparison extends over greater limits. Mr. Griffith considers the great bogs as due to these accumulations of gravel, which, by damming in the water, facilitate the growth of *Sphagnum palustre*. Under the bogs are deep deposits of marl, underlaid by clay and gravel, which further support the idea of ancient lakes. The marl was stated to be in one instance forty feet

thick. Mr. Griffith, confining himself on this occasion to the sedimentary rocks, commenced his illustrations by those of a more crystalline character, such as gneiss, mica slate, &c.; and stated that he considered the great groups of Ireland as corresponding to those of Scotland, particularly the Northern to the Grampians, and the Mourne to the Dumfriesshire mountains. The general direction of stratification is N.E. and S.W., though in Tyrone it is more nearly N. and S., being referred to a local axis; and in the south nearly E. and W. The beds of primary limestone, associated with the primary schists, are not continuous, though they occur in lines: when intersected by trap dykes, they become dolomitic. The quartz rock, which is also associated with these schists, is sometimes very remarkable. At Dunmore Head it has the structure of orbicular granite, or of some varieties of trap, for which it is often mistaken. Mica slate is unequally distributed: it is abundant in the north and west, less general in the south, and deficient in the Mourne or Down district. Mica also, as a mineral, is not general, being in the Mourne mountains often replaced by hornblende. Proceeding to the transition schists, Mr. Griffith stated his conviction that they would require subdivision, whenever materials had been collected for the purpose, in the same manner as those of Wales had been divided by Mr. Murchison. For example, in the older schists, neither conglomerates nor organic remains are found. In the newer greywackes, the slates alternate with sandstone; and again, in the still newer strata, limestone, containing fossils, alternates with the upper portion of the schists. The old red sandstone is also considered by Mr. Griffith divisible into two or three subsections, the upper alternating with the mountain limestone. Mr. Griffith then described the several coal-fields of Ireland, pointing out the distinction between those of the north and south, bituminous coal being confined to the northern collieries. The more recent sedimentary rocks were then briefly described, more especially the new red sandstone, which underlies the lias and chalk on the S. and E. of Antrim, and is also found in Monaghan, and may be traced thence through Tyrone and Derry to Lough Foyle, and round Lough Foyle to Donegal.

Having previously described the sedimentary, he now entered on a description of the crystalline rocks considered as rocks of intrusion. In the Wicklow range, extending to Brandon, the granite contains no hornblende, and, as previously noticed by Mr. Weaver, occurs sometimes as beds in mica slate. In the Mourne or Down range, the granite contains hornblende, which frequently predominates over the mica. In Wicklow, mica slate, passing into gneiss and clay slate, abuts without disturbance against the granite. In Down mica slate is wanting, and the other schistose rocks are frequently disturbed. In western Donegal mica slate and quartz rock are abundant, the quartz rock being developed to a great extent; and in Galway also, associated with mica slate, quartz rock is extensively diffused. In both these counties granite occurs, and the crystalline stratified rocks are referred to as affording distinctive characteristics of its several localities. The phænomena usually exhibited by granite

veins are frequently observable, such as their passage through the adjacent schists, detached portions of which are often enveloped in their substance, and the change they effect in their structure. Mr. Griffith next described the older and newer trap districts, mentioning many interesting particulars connected with them, such as the capping of quartz rock by greenstone, the concentric arrangement of the beds of greenstone in Donegal, and the occurrence of quartz rock between two beds of greenstone, the quartz being columnar, the trap, above and below it, not. In Slieve Gullin greenstone and granite were stated to be actually mixed together, whilst in Carlingford the contact of the sienite (or greenstone) with the granite is concealed by debris. After noticing briefly the ochre beds which so often separate the beds of basalt, and expressing his belief that the trachytic porphyry of Sandy Brae in Antrim was nothing more than this ochre indurated by heat, Mr. Griffith adduced the fact of beds of sienite traversing the cliffs of Murloch Bay, and containing detached portions of chalk, as proof that the sienite was posterior in appearance to the chalk; and gave it as his opinion that all the crystalline rocks had been fused, and in most cases projected from beneath through the sedimentary rocks, the appearance of regular strata being due to their projection in a direction parallel to the strike of the beds.

Mr. Griffith stated the existence of an extensive marl deposit in Wexford, some of the shells of which appeared to correspond with those of the crag.

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*On a small isolated Patch of Granite which occurs in the County of Cavan. By Lieutenant* STOTHERD.

The superficial extent of this granite is about seven square miles, and it is separated from the nearest group of primitive rocks, that of the Mourne mountains, by the grauwacke or transition schists. This small district is entirely surrounded by transition and secondary rocks, and exhibits all those changes in the structure of the sedimentary rocks which are usually observed on their approach to, or contact with, rocks of a decidedly igneous origin, the schists becoming indurated and often changed into quartz rock. The appearance of primary rocks so far removed from any of the greater masses is extremely important in geological speculation, and assists in this instance in explaining the broken and detached character of the schistose hills, and the induration of many of their strata, since it is probable that the granite is at no great distance from the surface in the whole space between the Cavan primary rocks and the Mourne mountains, of which they might be considered an extension.

Copies of a map of the geology of the environs of Dublin, accompanied by a memoir, were presented to the Section by Dr. SCOULER, Professor of Geology to the Royal Dublin Society.

*On Eleven Trap Dykes in the Counties of Mayo and Sligo, running East and West for great distances. By Archdeacon* VERSCHOYLE. (Printed in Proceedings of Geological Society.)

*On certain Fossil* Polyparia *found in Alluvial Deposits in the vicinity of Limestone Hills. By Dr.* JACOB.

The specimens were *Lithodendra*, of the species usual in the carboniferous limestone of England, the coralline lamellæ being replaced by silica, and the limestone partially removed by water containing carbonic acid. Similar cases are frequent in the North of England: the circumstances under which they occur appear to Dr. Jacob to deserve special inquiry.

*On the* Silurian *and* Cambrian Systems, *exhibiting the order in which the older Sedimentary Strata succeed each other in England and Wales. By Professor* SEDGWICK *and* R. I. MURCHISON, *V.P.G.S.*

Mr. Murchison described a great group of fossiliferous deposits which rises out from beneath the old red sandstone. To these rocks, which he has termed in descending order the *Ludlow*, *Wenlock*, *Caradoc*, and *Llandeilo* formations, (each distinguished by peculiar organic remains, and frequently by subordinate limestones,) it was found essential to assign a comprehensive term, since they constitute one natural system interpolated between the old red sandstone and the slaty rocks of Wales. He observed that it was well known to all practical geologists, that in consequence of the recent advances of the science, it was absolutely imperative that the term "transition", under which such rocks would formerly have been described, should now be abandoned, since it had been so used, both by Continental and English writers, as to embrace the whole carboniferous series, from which the system under review was not only separated by the vast formation of the old red sandstone, but was specially to be *distinguished* by its fossil contents. Urged, therefore, by many geologists to propound an entirely new name for the class of rocks which had engaged his attention during the last five years, Mr. Murchison recently suggested (See Lond. and Edinb. Phil. Mag., July 1835, p. 48.) that the group should be termed the "*Silurian System*," the name being derived from the ancient British people, the Silures, who under Caractacus made so noble a stand against the Romans, and within whose territory the rocks under consideration are fully displayed. Mr. Murchison then pointed out, that wherever the limestones and typical characters of particular formations were absent or obscure, it was always practicable, over a region of 120 miles in length, extending from the neighbourhood of the Wrekin and Caradoc hills, in Shropshire, to the west coast of Pembrokeshire, to separate the groups into two parts, the "Ludlow" and "Wenlock" formations, forming the "*Upper Silurian*,"

the "Caradoc" and "Llandeilo" the "*Lower Silurian rocks*". He further remarked, that in South Wales he had traced many distinct passages from the lowest member of the "Silurian system" into the underlying slaty rocks, now named by Professor Sedgwick the "*Upper Cambrian.*"

This communication was illustrated by Ordnance Maps extending over large parts of eleven counties, coloured in the field by Mr. Murchison.

Professor Sedgwick commenced by pointing out the imperfection of the sections exhibited in the North of England, and some portions of North Wales, in consequence of the entire want of continuity between the carboniferous series and the inferior schistose groups. Some of the latter are fossiliferous both in Denbighshire and Westmorland; but in the interrupted sections of those counties it is impossible to tell how many terms are wanting to complete the series to the old red sandstone and carboniferous limestone. In the country described by Mr. Murchison these difficulties do not exist, and his sections have filled up a wide chasm in the succession of British deposits. Professor Sedgwick then described in descending order the groups of slate rocks, as they are seen in Wales and Cumberland. To the highest he gave the name of *Upper Cambrian group*. It occupies the greatest part of the chain of the Berwyns, where it is connected with the Llandeilo flags of the Silurian system, and is thence expanded through a considerable portion of South Wales. In one part of its course it is based on beds of limestone and calcareous slate; but on the whole, it contains much less calcareous matter than the Silurian system, and has fewer organic remains. Beds of good roofing-slate occur, and a perfect slaty cleavage is often observed in it transverse to the stratification; but other parts of it are of a coarse mechanical texture. To the next inferior group he gave the name of *Middle Cambrian*. It composes all the higher mountains of Caernarvonshire and Merionethshire, and abounds in fine roofing-slate, alternating with, and apparently passing into, irregularly interstratified masses of porphyry. Some portions of it are coarse and mechanical, and it contains (for example, at the top of Snowdon,) a few organic remains, and a few examples of highly calcareous slates, but no continuous beds of limestone. The same group, with the same mineral structure, and in the same position, but without organic remains, is greatly developed in Cumberland. The *Lower Cambrian* group occupies the S. W. coast of Caernarvonshire, and a considerable portion of Anglesea: it consists chiefly of chlorite schist, passing here and there into mica schist and slaty quartz rock, and contains subordinate masses of serpentine and white granular limestone. It contains no organic remains. Beneath the *Middle Cambrian* system (above described) there occurs in Cumberland (for example, Skiddaw Forest,) a great formation of dark glossy clay slate, without calcareous matter, and without organic remains. It passes in descending order into chiastolite slate, mica slate, hornblende slate, gneiss, &c., which rest immediately on granite. Whether the *Lower Cambrian* was to be placed on the exact

parallel of these masses in Skiddaw Forest, the Professor did not determine.

Professor Sedgwick explained the mode of connecting Mr. Murchison's researches with his own, so as to form one general system. He pointed out also the limit, as at present known, of fossils, none having been hitherto discovered in the *Lower Cambrian* schists, and remarked in reviewing the general phænomena, that geological epochs were not effected by shocks, but, like everything in nature, were under the dominion of the usual laws of causation.

*Notices of the Geology of Spain. By Dr.* TRAILL.

The author gave a sketch of the results of his personal researches in the geology of Spain, restricting himself, however, to a few only of the more striking peculiarities. He stated that it was an error to suppose all the mountain chains of Spain branches of the Pyrenees, from which they are in many cases completely separated. The variety of climate, and circumstances produced by the union of these mountains with the elevated table lands of New Castile, which is two thousand feet, and of Arragon, which is two thousand five hundred feet above the sea, had very peculiar effects on the flora of the country. Dr. Traill pointed out the identity of character which existed between the granites and schists of Spain and England, and proceeded to the newer strata; described the brine springs and salt lakes of Andalusia, and the deposit of salt which forms part of the base of the plain of Grenada. He also showed that lias and true chalk, with layers of flint, occur in the South of Spain, and confirmed the statements by Colonel Silvertop, of the tertiary deposits of Spain. Dr. Traill further observed, that bones are found in the fissures of other hills in Spain besides that of Gibraltar.

*On certain Disturbances in the Coal Strata of Yorkshire having a remarkable Relation to existing Valleys; illustrated by a Map and Sections. By* HENRY HARTOP.

M. AGASSIZ presented the fourth and fifth livraisons of his work on Fossil Fishes, and stated, that by the great addition of 300 species which had been obtained from the cabinets of these countries, the total number had been raised to about 900. He then advanced some general views on the conclusions to be drawn from the geological distribution of fishes, and explained the precision in determining epochs which their higher state of organization and consequent susceptibility to external influences afforded. The fishes of the carboniferous period were different from those of the lias; the fishes of the lias different from those of the oolite; and those of the oolite from the fishes of the chalk: and as it must be presumed that fishes living together so coexist from the necessity of their organization, and its adaptation to attendant circumstances, it must also be presumed that their disappearance was the result of a change in the

conditions of the earth's surface. In estimating the effects of such changes, it is necessary, M. Agassiz observed, to distinguish between general phænomena affecting, as it were, the laws of nature, and those of a mere local character, such as volcanic eruptions. The local phænomena may indeed have been similar to those of the present time, but the elevations of mountain chains are evidences of a more general class of phænomena, which have affected organic life, constituting thereby the various zoological epochs which may be traced in the earth's strata. It was in such periods of violence and change that the beds of any one system were deposited, the animals coexisting at the time being, according to the more or less susceptible nature of their organization, more or less completely annihilated; and it was in the tranquillity which followed, that new beings were formed, and lived to tenant in like manner the strata of another system, which should result from another epoch of disturbance. M. Agassiz produced, as an example of sudden destruction, a drawing of fossil fishes crowded together in a very confused manner, such as could only have arisen from an instantaneous catastrophe, arresting them, as it were, in a moment.

M. Agassiz then, at the request of Professor Sedgwick, explained those characters, such as the position of the fins, the arrangement and size of the scales, &c., by which the fishes of different geological eras may be distinguished, referring especially to those of the old and new red sandstones.

1. *On British Fossil* Astacidæ, *their Zoological and Geological Relations.* 2. *On British Belemnites. By* John Phillips, *F.R.S., G.S., Professor of Geology in King's College, London.*

[The leading results of these two communications, which form part of a general investigation of British organic fossils, undertaken at the request of the Association, will be given in the next volume of Transactions.]

*Notice of a newly discovered Tertiary Deposit on the Coast of Yorkshire. By* John Phillips, *F.R.S., &c.*

Two hundred yards north of the harbour of Bridlington, near the situation where Professor Sedgwick and the author and other observers had *suspected* and *looked for* tertiary beds, a wasting of the low cliff had disclosed to a small extent layers of greensand and clay, both, but especially the former, containing shells, &c. Diluvial clay and pebbles cover and partially confuse this deposit. Of 55 species of fossils from these beds, which are in Mr. Bean's cabinet at Scarborough, a very small number (four) belongs to the crag, a very small number (five or six) to recent species, and the greater proportion is extinct. On comparison of the facts known concerning this deposit, the crag, the Touraine beds, and certain other foreign tertiaries, Professor Phillips founded an argument concerning

the limits of error in the application of Mr. Lyell's test of the age of tertiary formations by the numerical relations of the species of fossils which they contain to recent forms. It appeared to Professor Phillips that these limits were wide, and that a method of such power and value must not be applied without great caution.

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A letter from Charles Lyell, F.R.S., Pres. G.S. to Professor Sedgwick, on the fossil shells of the Suffolk Crag, considered in two divisions, according to the views of Mr. Charlesworth, was read to the meeting.

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*Account of Fossil Trees in the attitude of growth in the Coal Measures near Glasgow. By* James Smith, *F.R.S., of Jordan Hill.*

The trees in question were discovered at Balgray Quarry, immediately adjoining the aqueduct over the Kelvin, about three miles to the north of the city of Glasgow.

The quarry abounds in the usual coal plants, laid horizontally; in one part of it a number of trees were found standing in an upright position, throwing their roots out in all directions, to all appearance in the attitude in which they grew, without fracture or disturbance. They rest upon, and are imbedded in, strata of sandstone, which are horizontal, or nearly so. The stems terminate about two feet above the roots, the superincumbent bed of stone passing over them as if they had been cut off. They are about two feet and a half in diameter, and are placed as near each other as trees of the same size could grow. No internal structure was observed, but from the ramification of the roots and of fragments of branches found near them, and the external appearance of the bark, which is channeled or furrowed, the author presumes that they were dicotyledonous.

*On certain Fossil Plants from the opposite Shores of the Bristol Channel. By the Rev.* David Williams, *F.G.S.*

These fossils were collected by the author in Devonshire and Pembrokeshire, from shales alternating with anthracitic coal (*culm*); and he states that, after a careful examination, he was led to conclude positively that the strata of the true localities belong to very different geological æras, that of Devon extending from Bideford to South Molton, being a true 'transition' coal, imbedded in 'transition' schists, and that of Pembrokeshire and Caermarthenshire belonging to the coal-measures above the mountain limestone. On the similarity of these plants found in formations of such different age, the author founds objections to the hypothesis of secular refrigeration; and the speculation that the atmosphere in early geological periods was charged with a greater quantity of carbonic acid gas; and proposes the case as at least an exception to the law, that strata may be identified by their imbedded organic remains.

*On the Survey of the Mersey and the Dee. By Captain* HENRY MANGLES DENHAM, *R. N., Resident Marine Surveyor of the Port of Liverpool.*

Captain Denham exhibited his trigonometrical survey of the Mersey and Dee, including the extensive sand-banks and channels of Liverpool bay, which, being delineated on the scale of four inches to the mile, afforded a detailed development of the submarine undulation, illustrative of his remarks on the action of the tidal stream in connexion with those differently shaped estuaries. The self-choking effects of the Dee, with its expansive mouth and gradual contraction, resembling a lateral section of a cone, were contrasted with the scouring effects of the Mersey, its contracted mouth and attenuated throat resembling a lateral section of a bottle with its neck pointed seaward. To this figure of the estuary of the Mersey, Capt. Denham ascribed the impetus of its expansive back-water, which has recently forced a channel of half a mile wide, and two miles long, and twelve and thirty feet below the low-water level, through sands, situated eight miles outside its coast-line confines, *at a tangent to its regular course.* Thus a most valuable and unexpected channel has been produced for navigation, and a compensating escape provided for its waters at a time when an injurious deposit was taking place across its usual path, where the efforts of the ebb become evanescent. The position was ascertained by Captain Denham to be fourteen miles below the docks, or tidal straits, where the first impulse amounts, (and continues so five hours out of six) to five miles per hour on spring-tides. The form of this channel corresponds to the contour of incidence and reflection throughout its whole course, and indicates the exhaustion of the velocity of the water by expansion in the proportion of 14 to 25. It proves also the certain power of the Mersey to command a *navigable* avenue to the ocean, so long as its guardians preserve the high-water boundaries from artificial contraction.

In the course of his professional duties, Capt. Denham proposes to himself a further investigation of the proportions of silt, &c. held in suspension and gradually deposited, as well as a determination of certain peculiarities in the *vertical* range of the tides with reference to atmospheric elasticity. He has already, by the liberal arrangements of the dock-trustees, been enabled to connect a series of observations, even to *five-minute grades*, during the twenty-four hours. From these, by extensive tabulary interpolations, the half-hourly rise and fall upon every stage of the moon was determined, and the mariner enabled at a glance to know what water existed in excess of his chart, and hence *when* certain subsidiary channels were passable, or the several banks might be crossed. He has thus ascertained the tidal *establishment*, or the time of high-water upon full and change of the moon, and determined another constant proportion as a standard—for graduating future tide-gauge operations, for testing soundings hereafter, for fixing a point of departure for engineers when levelling eminences, canals, railroads, &c.,—viz. the oscillating point, or mean centre which every six hours is common to neaps

and springs, and quoted by seamen generally as the *half-tide mark.* Capt. Denham is not as yet prepared to state whether some *small* constant difference might not be found as to the *instant* of the half-elapsed time of spring-tide, high and low water, and that of neaps, producing the actual *half-range* of tide to *inches*; but so satisfied is he of a closer approximation than is generally allowed, that, though he would never propose *to adjust soundings to that half-tide level,* because the mariner would have to make variable allowances to ascertain the least water he was to expect in the channel before him, yet he would suggest for scientific and frequent practical references the desirability of engraving on some rocky spot of every harbour, and sheltered portions of coast, the well-defined *half-tide level,* DATED; for, on the assumption that such a level is (no matter what the whole amount of rise and fall differs), in the same latitude, equidistant from the earth's centre, then we have a standard of obvious importance to science. By reference to this constant level those discrepancies may be adjusted which attend engineering operations, designed to cooperate on opposite sides of an isthmus, where the vertical range differs, and either *high* or *low* water level *separately* be started from, instead of the mean centre of *each range*, i.e. *half-tide* level.

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The Rev. WM. WHEWELL made the following remarks for the purpose of exemplifying the application of physical science to geological researches.

1. The permanence of the level of mean water, which Capt. Denham has recently proved by trial at Liverpool, suggests the proper mode of making such observations on the permanence of the relative level of land and sea, as were formerly recommended by the Association. In tidal seas the level of the ocean must, for such a purpose, be estimated with reference, not to the height of high or of low water, which is variable on many accounts, but to the height of *mean water.* This mean water is to be obtained by taking at least two high waters and the intervening low water, or two low waters and the intervening high water. A very few tides will give a near approximation to the true mean level; but the more there are taken, the more accuracy will be obtained. This mean level must, of course, for the purposes now spoken of, be referred to some durable mark in the solid ground. 2. The phænomena of terrestrial magnetism, being apparently connected with the internal constitution of the earth, are of interest to the geologist. According to the most recent researches of Hansteen the earth has four magnetic poles, all of them revolving in the neighbourhood of the geographical poles; and the periods of these revolutions are respectively about 4[illegible]00, 1740, 1300, and 860 years. These times, though long as historical periods, are short compared with many of those cycles of which geological researches and astronomical calculations prove the existence; and it is impossible not to feel a great curiosity respecting the nature of the subterraneous changes which take place in such periods. It concerns the geologist therefore, no less

than the physical philosopher, to further the progress of our knowledge of terrestrial magnetism. 3. The heat of the interior parts of the earth has always been treated of by those who have established the theory of heat upon mathematical principles. They have hitherto considered it as proved, upon such principles, that the increase of temperature of the substance of the earth as we descend, proves the reality of an *original heat.* But M. Poisson, in his *Theorie de la Chaleur* just published, dissents from this opinion, and is disposed to assign another reason for the higher temperature below the surface. He observes that the cosmical regions in which the solar system moves have a proper temperature of their own; that this temperature may be different in different parts of the universe; and that if this be so, the earth would be some time in acquiring the temperature of the part of space in which it has arrived. This temperature will be propagated gradually from the surface to the interior parts. And hence, if the solar system moves out of a hotter into a colder region of space, the part of the earth below the surface will exhibit traces of that higher temperature which it had before acquired. And this would by no means imply that the increase of temperature goes on all the way to the centre. Though these opinions may not gain the assent of geologists, it may be proper that they should be aware that such have been promulgated.

*On the Geographical Position of Cape Farewell. By Dr.* WEST.

The chief object of the memoir was to show, That Cape Farewell, so named by Davies in 1585, is not, as stated by Egede, Crantz, and Giesecke, on the island of Sermesok, but on another island many miles to the south-east of it;—That Staten Hoek is not, as generally laid down in charts, a promontory on the southernmost extremity of the main land, nor yet, as stated in the Edinburgh Review (No. 59,) an *inlet*, but that it is identical with Cape Farewell, and received its name, which signifies the *States' Promontory*, from the Dutch navigators. Dr. West also showed that this fact, though now apparently quite unknown in these countries, was understood and plainly stated nearly ninety years ago in an English work, Drage's Account of the Voyage in the California in 1746 and 1747.

The memoir was accompanied by a copy of Graah's Chart of Greenland, the latest and most correct extant, from which it appeared that Giesecke, in his account of Greenland in Brewster's Edinburgh Cyclopædia, and in his map of that country in the 14th vol. of the Transactions of the Royal Irish Academy, has placed the island of Sermesok nearly a degree too much to the south; that no part of the main land could possibly be seen from the open sea to the south of the coast of Greenland; and that the island most to the south of the strait Ikareseksoak is the only one on which is a cape answering to the description given by navigators of Cape Farewell.

Dr. West concluded his memoir by expressing his opinion that Captain Graah, by his having satisfactorily ascertained that there

was no trace whatever of a colony on the east coast from its southernmost extremity to lat. 65° 30′, has completely established the correctness of the opinion of Eggers that the Œsterbygd, or eastern settlement, was situated on the south-west coast, in what is now Julianeshaab's District; and that it received its name merely from the fact of its being to the east of the other settlement, the Vesterbygd.

## ZOOLOGY AND BOTANY.

### *On the Principles of Classification in the Animal Kingdom in general, and among the Mammalia in particular. By Professor* AGASSIZ.

Although the principal groups of animals are impressed with such characters as to be easily recognised and to admit of little doubt, yet their order and succession have been determined *by no general principle*. This appears from the discrepancy in the position assigned to them by the most eminent systematists, each of whom has assumed *arbitrarily* some organ or system of organs for the basis of his arrangement. Professor Agassiz, after adverting to some German naturalists who alone have sought after a general principle which should be satisfactory to "philosophic naturalists," passed in review the classes of the animal kingdom, each of which, he stated, exhibited in an eminent degree the development of some one of the animal functions. While Vertebrate animals (with Man their type) arrive at the greatest perfection in the organs of the Senses, the Invertebrate offer in the class of Worms the representative of the system of Nutrition, in *Crustacea* of Circulation, in Insects of Respiration, and in *Mollusca* of Generation. The Professor next proceeded to demonstrate in what manner each subclass of vertebrate animals derives its peculiar character from some one element of the animal œconomy.

This predominant element is the bony skeleton in Fishes, the muscular structure in Reptiles, the sensibility of the nervous system in Birds, and the perfection of the senses in *Mammalia*, which therefore reproduced the distinguishing character and constitute the type of vertebrate animals. He next showed that each of the other subclasses of the higher group is represented among the *Mammalia* along with its own peculiar type. He explained his reason for the fourfold division which he had adopted in the subclass, pointing out the close affinity which connects the *Ruminantia*, the *Pachydermata*, the *Rodentia*, the *Edentata*, and the herbivorous *Marsupialia*, (in none of which is the true canine tooth developed,) which he considers as forming a single group; in another he unites those characterized by the presence of the canine tooth in its proper function (as an instrument of nutrition, not merely of defence), viz. the *Carnivora* and those *Marsupialia* which partake of their character, and the *Quadrumana*. The *Cetacea* form a group in themselves;

and Man another. The manner in which these represent the subclasses of *Vertebrata* was exhibited by the comparison of

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| *Cetacea,* | with | Fishes, |
| *Ruminantia,* &c. | | Reptiles, |
| *Carnivora,* &c. | | Birds; |

while Man is the perfection and type of the mammiferous conformation.

Prof. Agassiz then applied this principle to illustrate the order and succession of the groups in *Mammalia* by a reference to the order in which the fossilized remains of the *Vertebrata* occur in the stratified deposits: 1. Fishes, 2. Reptiles, 3. Birds, 4. Mammalia. From the same consideration results the following arrangement of the representative groups among these last: 1. *Cetacea,* 2. *Ruminantia,* &c., 3. *Carnivora,* 4. Man, who thus in a twofold aspect becomes the culminant point of the animal creation.

*Observations on the Zoology of the Island of Rathlin, off the Northern Coast of Ireland. By* JAMES DRUMMOND MARSHALL, *M.D.*

The zoology of Rathlin does not offer any new species in addition to those hitherto found on the opposite coast of the county Antrim, and this notice was laid before the Association rather to mark the *habitats* of some species than to add anything to what is already known.

The only *Mammalia* frequenting the island are, the Norway Rat, the Common Mouse, the Shrew Mouse, and the Hare. The latter is but rarely seen, and not being able to procure a specimen, the author cannot say whether it is the hare of Great Britain or that lately ascertained to be a species, or rather perhaps a *variety,* peculiar to Ireland.

In *Ornithology,* so far as the author could ascertain, there are about 60 species, comprising 32 land and 28 water birds. From the situation of the island, its precipitous cliffs, and the consequent facilities for incubation, many species of water birds choose it for a summer residence. The most common species are the *Larus Rissa, Larus argentatus, Larus Canus, Alca Torda, Fratercula arctica, Uria Troile, Uria Grylle, Phalacrocorax Carbo, Phalacrocorax cristatus.*

Although all the above-mentioned species are plentifully distributed, the *Larus Rissa,* or Kittiwake, is by far the most numerous; every headland round the northern shore of the island was tenanted by this common though beautiful species. In company with it were found the *Alca Torda, Fratercula arctica,* and *Uria Troile,* all living in harmony with each other; the Puffins occupied the earthy patches which here and there occurred between the basalt and limestone of which the rocks are chiefly composed, while the three former tenanted every pinnacle and ledge of rock not otherwise occupied. The *Uria Grylle* inhabited one of the headlands on the

southern extremity of the island; but their numbers were by no means equal to those of the *Uria Troile* or *Arca Torda*. The myriads of fry of different species of fish, particularly the *Launce*, or Sand-eel, furnish an ample supply of food to the various sea-fowl frequenting Rathlin.

The *Fishes* of this island do not differ from those found on the northern shores of Ireland. One of the most common species is the Coal-fish (*Gadus carbonarius*). This on the Irish coast is called, in its different stages of growth, *Pickoc*, *Blochan*, *Glashan*, and *Grey Lord*, and corresponds, according to Dr. Neill, to the *Sillock* and *Piltock* of Shetland, the former name being applied to the fry, and the latter to the fish when a year old.

The Cod-fish is but rarely procured, there being but one cod-bank (which is called *Skirnaw*), lying between Rathlin and Isla in Scotland.

The Lithe, Ling, Plaice, and Turbot are occasionally caught; and during summer the Grey Gurnard and one or two species of Wrasse are plentiful round the shores.

The Fifteen-spined Stickleback (*Gasterosteus spinachia*) has been found in the pools on the shore, and in the rivulets and ponds the Short-spined Stickleback (*G. brachycentrus*).

## *Notices of the Geographical Range of certain Birds common to various Parts of the World but principally to India and Europe. By Lieut.-Col.* W. H. SYKES, *F.R.S.*

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| *Circaëtus brachydactylus*, Vieillot | India and France. |
| *Aquila chrysaëta* | India and Europe. |
| *Falco Tinnunculus* | India and Europe. |
| —— *Chicquera* | India and Cape of Good Hope. |
| *Circus cyaneus* | Europe, and only slightly differing in India. |
| *Strix Javanica* | India, Java, and Cape of Good Hope. (Very like *Strix flammea* of Europe.) |
| A Swallow hardly distinguishable from *H. rustica* of Europe | India. |
| *Alcyon Smyrnensis* | Smyrna and India. |
| *Alcedo rudis* | Dukhun and Cape of Good Hope. |
| *Muscipeta* (longtailed white and chestnut) | South Africa and India. |
| *Collurio Excubitor* | Europe and North America. A species or variety in India very slightly different. |
| *Oriolus Galbula* | Europe, India, and Cape of Good Hope. |
| ——— *melanocephalus* | India and the Cape. |
| Cape Thrush (*Ixos Caffer*) | India and the Cape. |

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| *Ixos falcatus* | Dukhun and Philippines. |
| Lesser Whitethroat | |
| *Budytes citreola* | |
| Stonechat | |
| *Phœnicura Suecica* | |
| *Emberiza melanocephala* | All found in Europe and India. |
| —— *hortulana* | |
| Common Sparrow | |
| *Pastor roseus* | |
| *Coracias Indica* | India and the Cape. |
| *Hoopoe* (not of Europe) | Cape and India. |
| *Leptosomus afer* | Cape and Dukhun. |
| *Cuculus fugax* | Eastern Islands and Dukhun. |
| *Centropus Philippensis* | |
| *Cuculus canorus* | Europe and Dukhun. |
| *Cinnyris aurucaria* | Cape and Dukhun. |
| —— *Mahrattensis* | Philippines and Dukhun. |
| *Columba risoria* | Senegal, India. |
| —— *œnas* | India, China, Europe. |
| Peafowl | Wild in India. |
| Common Fowl | Ditto. |
| *Coturnix dactylisonans* | China, India, Cape, Arabia, Barbary, Europe. (Not migratory in India and the Cape.) |
| *Pterocles exustus* | Asia Minor, India. |
| *Francolinus spadiceus* | Madagascar and India. |
| Several species of Herons | Common to India, the Cape, and Europe, or to two of these countries. |

The Sacred Ibis of Egypt is believed by Col. Sykes to be the same as the Indian Ibis.

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| *Ibis falcinellus* | Europe and India. |
| Green Sandpiper | |
| Wood Sandpiper | Common to Europe and India. |
| Common Sandpiper | |
| *Totanus Ochropus* | Hudson's Bay and India. |
| Common Snipe | India and Europe. |
| Jack Snipe | |
| *Rhynchæa* | Cape and India. |
| *Pelidna Temminckii* | India and Europe. |
| *Jacana* | China and India. |
| *Gallinula* | Java and India. |
| *Porphyris* | Madagascar and India. |
| Coot | Europe and India. |
| *Cursorius Asiaticus* | India and Cape. |
| Golden Plover | N. America, Europe, India. |
| *Himantopus melanopterus* | Java, India, Europe. |
| *Anas strepera* | |
| *Rhynchaspis virescens* | India and Europe. |
| *Mareca fistularis* | |

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| *Querquedula circia* ———— *Crecca* *Fuligula rufina* ———— *cristata* | India and Europe. |
| *Sterna Anglica* | North coasts of Great Britain and Dukhun, 100 to 200 miles inland, and 1800 feet above the sea, with similar changes of plumage from summer to winter. |

Besides the instances of *identity* above quoted from specimens in Colonel Sykes's own cabinet, others are mentioned of such *close analogy* as to render their specific difference extremely dubious. Many species of birds of different natural groups and habits are thus proved to have an extensive geographical range, under considerable differences of mean temperature. Deducting those species, which do or may be imagined to migrate from one region to another, there remains abundant evidence, derived from continually resident birds, that some birds live in India with a mean temp. of 77° to 82°, and in Britain with a mean temp. of 45° to 50°. Connecting these facts with the instances of tigers living near the limits of perpetual snow, and elephants and Indian birds braving our winters, Colonel Sykes concludes that the power of acclimation possessed by many birds and other animals is very considerable, and capable of useful application to a question of practical importance, viz. the necessity of employing artificial heat generally in our vivariums, and to the curious geological problem of the climate of the globe when elephants and tigers were inhabitants of the northern zones.

[Captain JAMES ROSS, in corroboration of these views, stated that the Stonechat, Whitethroat, and Golden Plover were inhabitants of Hudson's Bay, and that the Raven also occurs in the Arctic Circle, without being subject to change of plumage.]

### *On the Infra-Orbital Cavities in Deers and Antelopes. By Dr.* JACOB.

[This paper having been drawn up in compliance with a recommendation of the Association, will be printed in the next volume of Transactions.]

### *On a Mode of preserving* Echinodermata. *By the Rev.* CHARLES MAYNE.

In the year 182[illegible] being at the sea-side, Mr. Mayne collected many *Echini* for examination; and the house not being large enough to afford him a separate room, he used chloride of lime to prevent inconvenience to the family from the smell. He soon perceived that the *Echini* steeped in the solution did not lose their spines; he accordingly tried to preserve them with all their spines on, and suc-

ceeded completely. He has since tried this process with many *Echini* and small star-fish. The preparation should not be so strong as to act sensibly on the surface of the crust, as in that case he found that the spines would fall off.

### *On* Pentacrinus Europæus *and a Species of* Beroë *taken in Dublin Bay. By* R. Ball.

Specimens of these were exhibited to the Meeting. The *Beroë* has been examined by Mr. R. Patterson of Belfast, who finds it to be a new species of the genus *Pleurobrachia* of Fleming. It has been also taken in Larne Lough, Antrim.

### *Account of a Toad found alive imbedded in a solid Mass of New Red Sandstone. By* T. L. Gooch, *Resident Engineer on the London and Birmingham Railway. Communicated by Mr.* Sturge.

The following is an abstract of the statements contained in this communication.

In the excavations for the London and Birmingham Railway, in the Park Gardens at Coventry, the earth was opened to a depth of eleven feet on the 16th of June 1835; the section presented soil eighteen inches, mixed sand and clay three feet, masses of red sandstone, somewhat severed by 'backs' and fissures, but requiring the use of iron bars, and occasionally powder. One of these masses, near the bottom of the excavation, having its three dimensions eighteen, fifteen, and five inches, being lifted and thrown towards a wagon, fell on the ground and broke nearly through the centre; the divided parts lay about an inch asunder. One of these fragments having been thrown into the wagon, a Toad was observed in a cavity or cell in the face of the remaining fragment, and was projected thence in consequence of the workman kicking the stone. The other fragment of stone being reapplied to its fellow, it was found that an oval cavity existed in the centre, which had no visible communication to the surface.

The cavity of the stone in which the Toad is said to have been imbedded was lined with a thin black deposit; on one side of the cavity, which was more rounded than the other, this deposit was most visible.

The colour of the Toad was at first *bright brown*; in ten minutes it had grown almost *black*: it seemed oppressed and gasped frequently; was rather under the usual size, but plump, and apparently in good condition, but seemed to have been injured on the head. It was replaced in the hollow of the stone, the crack having been stopped with clay, and died in four days.

The Rev. Dr. Drummond stated that, from observations lately made by him, the *Gordius aquaticus* seems to be viviparous.

## *On the Action of Light on Plants. By Professor* DAUBENY.

Professor Daubeny reported the progress which he has made in his experiments on this subject since 1833, when he communicated the results obtained up to that time to the British Association at Cambridge. At that period he had ascertained that the quantity of carbonic acid decomposed by a plant was in proportion, not to the chemical or heating influence of the ray transmitted to it, but to its illuminating power: he has since found that the functions of exhaling moisture by the leaves, and absorbing it by the roots, depend upon the same law; with this difference, however, that, provided some light be present, a body radiating much heat will serve as a substitute for one transmitting a greater degree of light. Thus, a solution of ammonio-sulphate of copper, which absorbs and consequently radiates much heat, is nearly as efficient in causing the exhalation and absorption of moisture as glass, which transmits the entire spectrum; and in proof that this does not depend upon any peculiar power residing in the violet ray, water obscured by ink, so as to produce an equally feeble illuminating effect, was found, in consequence of the heat it radiated, to produce an equal degree of exhalation. Yet when the plant was covered over by opake bodies radiating much heat, the amount of moisture exhaled was very inconsiderable.

Professor Daubeny has employed, in his experiments on plants, the light emitted by balls of lime ignited by the oxy-hydrogen jet, but could not discover that it exerted any influence on the quantity of moisture exhaled by them.

## *Observations on the Structure of Horizontal Branches of* Coniferæ. *By* WILLIAM NICOL.

In a paper on the structure of recent and fossil *Coniferæ*, inserted in Professor Jameson's Philosophical Journal for January 1834, the author gave an account of a very striking difference he had observed in the structure of the opposite sides of a piece of the wood of *Taxodium disticha*. The pith was much nearer one side than the other, and the narrowest was of a paler colour than the broadest side. The narrow side showed the usual structure of the true Pines in all the three principal sections, but the broad side in the transverse section possessed a greater degree of solidity than the narrowest side, and in both the longitudinal sections the vessels were filled with decussating fibres, and the discs were not only more sparingly bestowed but were also smaller and more obscure than those occurring in the other side. At the time this wood was examined he did not know whether it was a portion of a stem or a branch. He has since ascertained that it was a horizontal branch, and it then became interesting to determine whether the difference of structures was peculiar to the piece of wood in question; whether it occurred in both the stem and branches of *Taxodium disticha*; whether it was peculiar

to that kind of wood; or whether it was a general feature in the horizontal branches of other *Coniferæ*.

The first step in the investigation was to procure another branch of *Taxodium disticha*. This he did last summer, and marked the upper side before the branch was cut off. The structure of this branch agreed in every respect with that of the branch formerly examined, and the pale-coloured or narrowest side was the uppermost. The next step was to ascertain whether the stem of *Taxodium disticha* agreed in structure with the branches. For this purpose the author requested Mr. James Macnab, of the botanic garden of Edinburgh, to bring him from America a portion of a stem. This he was so kind as to do last winter. The stem was five inches and three tenths thick in the longest diameter. The pith was nearer one side than the other by three quarters of an inch. The surface of the cross section was of a uniform pale colour, with the exception of a spot surrounding the pith nearly an inch in diameter, of a slightly darker shade. On examining a number of sections of this stem, they were all found to agree with coniferous stems in general, and showed not a trace of the structure occurring in the under side of the horizontal branches.

Having thus ascertained that in *Taxodium disticha* the difference of structure alluded to was peculiar to the horizontal or nearly horizontal branches, the third step was to determine whether any other coniferous horizontal branches agreed in structure with those of *Taxodium disticha*. With this view Mr. Nicholas lately procured branches of ten different species of Pines, and has found them all agreeing in structure with those of *Taxodium disticha*. The pith is always nearer the upper than the under side. The upper or pale portions have discs similar to those of the stems, and show no trace of decussating fibres in the vessels or spaces containing the discs. The under or darker-coloured portions have fewer, smaller, and more obscure discs than those contained in the upper part, and the spaces between the vertical partitions in both the longitudinal sections have decussating fibres, which, however, are often finer and more crowded than those in *Taxodium disticha*.

It may be right to remark, that in coniferous horizontal branches the pith is always more or less eccentric, and that in some instances the eccentricity is great. In a branch, for example, of the black spruce, the cross section, which is somewhat oval, has a vertical diameter of three inches and three tenths. The distance of the pith from the upper side is only half an inch, and from the under side it is two inches and eight tenths. There are thirty distinct annual layers in the under side; but these thirty layers, when crowded into the space of half an inch in the upper side, are so minute that they can scarcely be enumerated. This, however, is an extreme case, the pith being in general less distant from the centre. The branches of some pines, particularly the larch, are nearly cylindrical, but even in these the pith is always out of the centre.

But although the upper and under sides of many, perhaps all, coniferous branches, present a different structure, yet such a difference

is not entirely confined to the branches. In some few stems a similar difference has been seen in the opposite sides. In a stem of *Pinus Cedrus*, for instance, one of the sides was of a pale colour, and had the usual structure; the other side was of a darker colour, and had a structure similar to that of the under side of horizontal branches. Another portion of the same kind of wood, however, was of a uniform colour, and had throughout the usual structure. A young stem of *Pinus laricia* had a structure similar to that of branches, and the same was observed in an upright stem of *Cupressus sempervirens*.

### *On the Formation of Wood. By Dr.* West.

Dr. West exhibited a specimen of Bog Yew, in which, from the non-adherence of two successive annual layers, the central portion of the heartwood, though in close contact with the surrounding portion, which constituted the greatest part of the bulk of the tree, was throughout its whole extent perfectly distinct from it, so as to present the appearance of a small tree which had grown up through the centre of a large one, adapting itself completely to its cavity. He considered this singular phænomenon to be the result of a severe frost, which had either frozen a very thin layer of alburnum, so as to destroy its vitality, and thus prevent the next-formed layer from adhering to it, or else, without absolutely destroying it, had so affected its exterior surface, as to produce the same result. He expressed a doubt whether this exactly answered to the lesion called by the French *gelivure*; and produced a drawing, copied from one by Decandolle, of a section of a juniper tree affected with that lesion, in which the diseased layer was of comparatively considerable thickness, whereas in his specimen there was no appearance whatever of a diseased layer, however thin, nor any space where such could have been. He alluded also to another lesion, mentioned by Duhamel, called *roulure*, which consisted in the non-adherence of the annual layers, and so far appeared to have a greater resemblance to the case under consideration; but for want of a more detailed account he did not venture to pronounce whether they were identical. He next entered into the consideration of how far this case, and still more that of Decandolle's juniper tree, might be urged in favour of Duhamel's theory of the formation of wood, and against those of Decandolle and Du Petit Thouars; and remarked that at all events it clearly proved that the bark can form good wood, independently of the aid of the alburnum. He further adduced the fact, that the nodules of wood that are found on the trunk of the beech have always a layer of liber interposed between them and the alburnum, and expressed his opinion that this afforded an additional proof, that the bark has, in general, if not the sole, at least the predominant influence in the formation of wood. In this specimen, the annual layer formed after the occurrence, whatever it was, that prevented its adhesion to that of the preceding year, was as

thick and sound as any of those that were near it, though it must apparently have been formed wholly by the liber.

*Notice of a Yew found in a Bog in Queen's County.* *By* CHARLES WILLIAM HAMILTON, *Honorary Secretary of the Horticultural Society of Ireland.* (*Communicated by Mr.* MACKAY.)

In this tree Mr. Hamilton was able to count annual rings or layers indicating a growth of 545 years. Yet so compact was the wood, or so close the layers, that the diameter of the trunk did not exceed a foot and a half, or its circumference three feet and a half. The growth had been very slow during the last three centuries, for near the exterior there were about 100 rings within the space of one inch.

Many years ago Mr. Mackay measured a yew tree, growing on the island of Innisfallen on the Lower Lake of Killarney, of nearly double the dimensions of the one described by Mr. Hamilton, or between six and seven feet in circumference.

*Notice of the Yew at Mucruss.* *By Dr.* LITTON.

Dr. Litton had tried the age of the celebrated yew tree at Mucruss by Decandolle's test, and found that the result nearly agreed with the tradition. He exhibited a specimen of an oak tree bearing the impress of letters on the inner concave surface.

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Mr. SAUNDERSON noticed a passage in an old Scotch history, which stated that the northern part of Ireland was so much infested by yew trees that a great emigration of Irish took place in consequence, who, with their families and cattle, went over to settle themselves in Scotland, the yew trees every year destroying their cattle in Ireland.

*On Bog Timber.* *By the Rev. Archdeacon* VIGNOLES.

The bogs of Westmeath are numerous, covering a considerable extent of the county. They almost invariably present the same natural appearance, only some are much more thickly imbedded with bog timber than others. In some of these there are three layers of trees to be found; and alternating with them as many layers of peat from three to five feet in depth. The trees in each layer appear to have arrived at maturity, and could not have been coexistent. The specimen of bark exhibited was taken from a tree 56 feet long; squaring from 2 feet to 18 inches: it lay upon a heathy bed; consequently where it fell the surface was heath. It was charred from top to bottom. With very few exceptions, all the timber found in the neighbourhood bear the marks of fire. The roots are rarely found attached to the tree, but likewise bear evident traces of having been burnt. They are of enormous size.

Dr. MARTIN BARRY communicated the result of some observations on the colour of the sky, as seen from the summit of Mont Blanc; and expressed his conviction, that, while the depth of this colour appeared very much increased, as might be expected, from his elevated position, its peculiar tinge of black was in a great measure due to the contemporaneous reception by the eye of rays from the snow. He stated that the same effect has been observed by Boussingault in his attempted ascent of Chimborazo and other mountains.

*Cursory Remarks upon some matters contained in a Letter addressed by Mr. William Hamilton to Mr. Packenham. By* WILLIAM SCHIEDE, *M.D.* (*Translated by Mr.* HAMILTON.)

1. The *Oxalis tuberosa* is a plant of Chili, not of Mexico; at least I have never heard of any plant of this genus with esculent roots being cultivated in the Mexican republic. The country abounds in wild species of *Oxalis* (the *Xoxocayullin* of Hernandez), some of which are applied to culinary purposes in the same manner as the Sorrels (*Rumex*) of Europe.

2. The *Solanum tuberosum* is, without doubt, a native of this soil, as has been already published in the beginning of 1829. I have collected several varieties, which may, perhaps, prove to be distinct species. Moreover, I have collected among them one species (*Solanum oxycarpum*, Schiede) equally tuberous, and in every respect akin to the *S. tuberosum*, from which it differs in bearing pointed fruit. Notwithstanding which, the *Papa*, according to my researches, has no Aztec name, being known to the Aztecs by the name of *Papa*. Hernandez speaks of the Peruvian *Papa*; which proves how little he was aware of its being a plant of this country.

3. According to my observations, the *Cevadilla* is a new plant (*Veratrum officinale*, Schiede). Hernandez has described and figured it very indifferently under the name of *Hzcuinpatli*, or Dog-killer. It is a powerful anthelmintic, diuretic, antiarthritic, and antipsoric. I am not aware of its having been employed in the cure of hydrophobia. In the course of the last ten years a new species of *Veratrum* (*V. Orfilia*, Sabadilla,) has been published by Descourtilz, which he conjectures to be the plant which yields the cevadilla of the shops of Europe. In my opinion, this last plant is doubtful, and is at least distinct from the *Cevadilla* of this capital and of the shops of Berlin.

4. I am not acquainted with the plant called *Amole*, of the province of Sonora. A root is exposed for sale in the market-place of Mexico, under the name of *Amole*, which is the *Agave polyanthoides* of Schiede, or at least one nearly related to it. It is commonly used for washing linen, in place of soap, as it abounds in an extractive and saponaceous principle.

5. I do not know the *Cestrum Mutisii*. If I mistake not, it is a production of South America. In some parts of Mexico they employ in its place the sap of the *Justicia tinctoria*; but I cannot say

whether or not its colour is as durable as that of the *Cestrum*, having written a number of letters with it to Europe, which are consequently not in my possession.

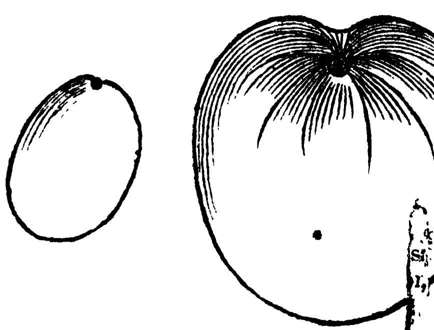
6. Should the *Huelosochil*, mentioned by Mr. Hamilton in his letter, be the *Yoloxochitl* of Hernandez, I can pronounce it to be the *Talauma Mexicana* of Jussieu. Its reputed medicinal properties are most probably exaggerated; and both the flowers and the seed are employed in this country in the cure of various nervous affections, and especially epilepsy. *Yoloxochitl* is an Aztec term implying the Flower of the Heart (*Flor del Corazon*).

I shall conclude these remarks by observing, that several of the matters indicated, together with a multitude of others relating to the vegetable kingdom of Mexico, have been discussed in a German botanical periodical: "Linnæa. Ein Journal für die Botanik in ihrem ganzen Umfange. Herausgegeben von D. F. L. v. Schlechtendal. Berlin, Jahrgang 1829, et seqq."

*Mexico, 28th December, 1834.*

Mr. Hamilton stated that he has lately received a pericarp and two nuts of the celebrated *Palo de Vaca*, from the vicinity of the farm of Barbula, spoken of by Humboldt; but, unfortunately, they did not reach him in a state fit for vegetation, and were apparently too old when gathered. He has written for a further supply of fresher fruit, and specimens of various ages, together with the flowers, which have never yet been botanically examined.

Below is a rude outline of the pericarp and one of the nuts, of their natural dimensions.



Pericarpii diameter equatorialis poll. 2; diam. polaris $1\frac{7}{8}$ poll.

He has also been favoured by Sir R. Ker Porter, with a few seeds of the wax-tree of Guyana, and a candle made from its wax. A plant raised from one of these seeds is now growing in Pontey's nursery at Dublin, but has not yet assumed an arborescent character. What this tree is cannot yet be ascertained; it bears at present little resemblance to the genus *Amyris*, to which it might otherwise have been suspected to belong.

## *On the Mathematical Relations of the Forms of the Cells of Plants. By Dr.* ALLMAN.

Having demonstrated the reciprocity of the five solid forms, viz. the sides and angles of the tetrahedron, the cube and the octohedron, the dodecahedron and icosahedron, the author endeavoured to reduce to corresponding systems the forms of the cells of plants.

The dodecahedron and icosahedron, considered as respectively forming the cellular tissue, appear, from the natural consequences, well to agree with, and so far to explain, sundry exterior appearances in groups of plants also in structure reciprocal, the *Exogene* and the *Endogene*.

The triangular solid angles of four ordinate dodecahedra may meet at a point, and leave exterior spaces; the quinquangular solid angles of four icosahedra cannot, without mutual encroachment, meet at a point, but must leave interior spaces.

If it be reasonable that the tubes or fibres of plants, whose growth is always posterior to that of the cells, be arranged where most room is afforded, or where least pressure is found likely to exclude them, the tube, or fluid of the tube, by the approximation to the sphere, or distension of the cell, would be driven from the middle of the side to the edge, from the edge to the solid angle. Five vertical planes may pass through all the solid angles of the dodecahedron; three such planes may pass through all the solid angles of the icosahedron.

If the central mass of approximate dodecahedra should be a little augmented before the tubes be established, the ordinate dodecahedra might easily pass into the rhombic, which are capable of forming, without interstice, a compact mass. For the solid angle (there being twenty like) of the ordinate dodecahedron is formed of three plane angles, each of 108°; the triangular solid angle of the rhombic dodecahedron (there being eight like, besides six quadrangular) is formed of three plane angles, each of 109° 28′; and this is the measure of each of the three plane angles which form the central solid angle of the tetrahedron. The measure, also, of each of the four plane angles which form the quadrangular solid angle of the rhombic is 70° 32′, the measure of each of the four plane angles which form the central solid angle of the cube: hence, six like solid angles accurately meet at a point.

Two vertical planes, perpendicular to each other, may pass through all the quadrangular solid angles, and through four of the eight triangulars of this rhombic, the four which remain being found in two other planes of the like direction.

It perhaps will not appear too subtile to refer—to a central cellular structure approximate to this, the Olives and others, binary in seeds, ovaries, stamens, corolla, calyx, branches, and leaves; adding, perhaps, the Wall-flowers and Celandines, somewhat reciprocal in the relative position of the trophosperms, as referred to different views of the horizontal central section of the same rhombic—

To the rhombic structure, with a shell of ordinately dodecahedral cells, the Nightshades, the Periwinkles, and many others—

To the dodecahedral throughout, Kalmias, Flax, Wood-sorrel, &c.—

To the dodecahedral half-twisted, the branches departing from the stem, in Horse-tails; varying divisions in the flowers of Jasmine and Clematis, quaternary and quinary, as noticed in others by Linné, and easily corroborated by many examples:

To this altered structure in all the flowers, *Centunculus*, *Radiola*, *Tormentilla*.

In general, to the icosahedral structure,—approximately, however, as we cannot understand icosahedra thus to mould each other,—the *Endogene* races of plants.

Dr. Allman briefly refers, as to a subsidiary solid capable of moulding, and of leaving within octahedral spaces, to the tetradecahedron, reciprocal of the rhombic, as the icosahedron is of the ordinate dodecahedron. The four plane angles which form the solid angle of this fourteen-sided solid, together measure 300°, as do the five plane angles which form the solid angle of the icosahedron. Four such quadrangular solid angles, with two quadrangular solid angles, each plane angle of 60°, as of the octahedron, accurately meet at a point.

Notwithstanding, this solid seems admissible into the structure *Exogene*, of which the examples among plants are far more numerous than of the reciprocal *Endogene*.

The author suggests its conformity to the square stems and four exterior distinct packets of fibres in the *Calycanthi*; viewed in different positions, its indication of the ternary ovary, with the quaternary exterior, in Soap-trees, and in *Tropæolum*, varied still more?

All the other above-named solids (except the ordinate dodecahedron and the icosahedron) may be derived from two tetrahedra.

Those of equal mean diameter, placed reciprocally at a common centre, have their envelope the cube, their nucleus the octohedron.

These last, of like dimension and position, have their envelope the rhombic dodecahedron, their nucleus the tetradecahedron.

Between the squares of the diameters of circumscribed and inscribed spheres, the square of the mean diameter is, in the tetrahedron a geometrical, in the cube an arithmetical, in the octohedron a harmonical mean. The continued proportions 9. 3. 1.; 3. 2. 1.; 6. 3. 2.

*On the Formation of a Natural Arrangement of Plants for a Botanic Garden. By Mr.* Niven.

The principal object of this plan is to divide the exotic from the European plants by a serpentine walk, bringing the allied species in juxtaposition by the numerous curvatures.

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*On Phænogamous Plants and Ferns indigenous to Ireland which are not found in England or Scotland. By Mr.* Mackay.

Mr. Mackay having been requested to present a general report on

this and other branches of the botany of Ireland at the next meeting of the Association, this communication is omitted, as well as other notices of the same nature by Mr. Babington, Mr. Curtis, and Professor Graham.

Various other notices connected with the subjects of the papers were received from Dr. Coulter, Professor Graham, Mr. Curtis, Colonel Sykes, Mr. Fox, Mr. Waterhouse, Mr. J. B. Yates, Dr. Traill, Mr. Haliday, and Mr. Marshall.

## MEDICAL SCIENCE.

### *On the Peculiarities of the Circulating Organs in Diving Animals. By* JOHN HOUSTON, *M.D., M.R.I.A., &c. &c.*

The circulation of the fluids in living animals, though mainly carried on by the influence of the vital powers, is nevertheless to a certain extent amenable to the general laws of hydraulics. Gravity, motion of the particles of the solids upon each other, the respiratory function, pressure on the surface of the body, all, under various modifications, promote or retard the movement of the fluids along their vessels. But of all the collateral circumstances exerting an influence of this nature, the action of the chest and lungs appears, in warm-blooded animals, to be one of the most important. Suspension of respiration puts a stop to the circulation of the blood through the lungs; this fluid under such circumstances stagnates in the vessels leading to these organs, and cannot pass forwards until air be freely readmitted: death in a few moments is the inevitable consequence of such interruption. Animals living in atmospheric air cannot exist under a state of suspended respiration so long as those whose natural habitation is the water. The most expert diver has never been known to remain submersed for more than two minutes at a time, whilst it is well known that the whale can remain under water for upwards of twenty. Now, the arrangement of the respiratory and circulating organs in man and cetaceous animals, and the influence of these two systems on each other, being the same, though their powers of suspending respiration with impunity are very dissimilar, we naturally inquire, on what does this latter difference depend?

Independently of the suspension to respiration which occurs in these animals whilst under water, there is another cause operating, when at great depths in the ocean, to the prejudice of their circulating fluids, such as is never experienced by terrestrial animals, namely, pressure on the surface of their bodies by the water, increasing with the depth from the surface. A boat, as observed by Scoresby, when dragged to the bottom of the sea by a whale into which a harpoon was struck, became in a few minutes as completely soaked in every pore as if it had lain at the bottom of the sea since the Flood: after being raised again to the surface, by the whale returning "to blow", it could with difficulty be got into the ship on account of its great weight; and a fragment of it, when thrown into the sea,

sank to the bottom like a stone. And are we to suppose that a degree of pressure under water, sufficient to soak in an instant every pore in the planks of a large boat, was not felt by the animal which dragged it to such a depth? There can be little doubt that the application of this pressure would repel the fluids from the vessels near the surface of the animal into those more removed from its influence in the deeper recesses of its body; that, in fact, an effect would follow, the opposite of that which is produced in an animal when placed under the exhausted receiver of an air-pump; or such as occurs in persons attaining so high an elevation in the atmosphere as to be freed from some of its weight, in whom the blood is determined to the surface, producing giddiness, bleeding from the nose, ears, lungs, &c.

We may consider, therefore, that aquatic mammalia can exist with impunity during periods of suspended respiration, and also under degrees of pressure which would be destructive to the lives of animals of the same class whose element is exclusively the atmosphere. And this may be considered still more remarkable, when it is recollected that during those periods of breathlessness and universal pressure, the voluntary and rapid movements which these animals perform when in pursuit of their prey, tend to urge towards the lungs, where the principal obstruction exists, all the fluids contained in the veins among the muscular structures of the body.

The object of Dr. Houston's communication is to point out the provision on which these peculiar diving faculties of such animals depend; a provision beautifully harmonizing with all our physiological notions, and admirably adapted to the end in view. It consists of reservoirs connected with the veins leading to the lungs, where the blood may find a temporary resting-place during the period at which the asphyxiated condition of these organs refuses it transmission through the vessels. Dr. Houston exhibited numerous preparations and drawings demonstrating the presence of this singular provision in the porpoise, seal, otter, great northern diver, gannet, &c. The veins principally concerned in these dilatations are those nearest the heart, *viz.* the venæ cavæ, the venæ [illegible]væ hepaticæ, the jugulars, the veins of the spine, and those in the posterior regions of the abdomen. In the seal the venæ hepaticæ form large bags in the liver; and in the same animal there is on the neck, and along the sides, and posterior part of the neck, a plexus of veins of such size, that, when they are filled with injection, the parts beneath cease to be visible; the vessels are as thick as the finger, and coiled, and heaped up on one another to an almost incredible amount. The contrast between the condition of the venous system in the great northern diver and that in the gannet, as exhibited by Dr. Houston, is important in establishing the uses of these reservoirs.

The diver and gannet are both seafaring birds, but differ remarkably in their modes of seizing the fish on which they feed. The diver swims under water after its prey, and remains for such periods long out of sight; the gannet pounces on it like an eagle, when discovered by its quick-sighted eye near the surface of the water. and thence

carries it up to some dry spot, impaled on its long, sharp bill. As might be expected in those two birds of such opposite habits, the provision of reservoirs for stagnant venous blood is largely developed in the one, but completely absent in the other. In the diver the venæ cavæ and venæ cavæ hepaticæ are dilated to a size equal to that of the same veins in the adult human body, and there is, moreover, a kind of second auricle, designed to render the provision more complete; whilst in the gannet these veins, and all the others in the body, are of the ordinary dimensions.

Dr. Houston made allusion to the habits of pearl-divers, and offered a conjecture that in those individuals, to whom practice has given such a power of remaining long under water, some dilatation of the venæ cavæ and venæ cavæ hepaticæ may be gradually effectuated, giving them their superiority over other men in suspending the breath, and approximating them thereby somewhat to the condition of aquatic mammalia. The dilatations which are known to take place in these vessels in some varieties of disease of the heart, he adduced in evidence of the possibility of such an occurrence.

*An Account of a Variety of Hydatid* (Cysticercus tennicollis) *found in the Omentum of an Axis Deer; with Observations on its Pathological Changes. By* JOHN HOUSTON, *M.D., M.R.I.A., &c.*

This hydatid, varying in size from an almond to an orange, generally single, sometimes in connexion with another, lies in a smooth membranous cyst between the layers of the omentum. Its head and body are in the living state inverted into the cavity of the caudal vesicle; but by immersion in tepid water they become visible, and are always found protruded and naked in hydatids which have undergone death before the decease of the parent animal. Dr. Houston considers that the inversion of the head is the natural condition, and that its eversion is the result of some irritation or of death. He also differs from most other helminthologists, in being of opinion, that the lateral depressions on the head, termed mouths, and visible only to the microscope, are covered over with a thin pellicle, and incompetent, therefore, to the office assigned to them, *viz.* that of being agents for the imbibition of nutriments, as he found that fluid squeezed from the vesicle in the direction of the head, protruded and rendered convex the membranes of these apertures before making its escape through them. Dr. Houston agrees in opinion with those who consider that the function of imbibition is carried on by the whole surface of the little animal. From the examination of the specimens of hydatids which existed in great number and variety in this case, the author has been enabled to describe and delineate the different stages of the process of degeneration, to which he considers all such animals are by their nature subjected; and has arrived at conclusions as to the seat of these degenerations different from those advanced by other authors. He considers, That the term allotted for the existence of each individual hydatid having expired,

the little animal dies, and in the dead state comes to act as a foreign body on the cyst which contained it;—That the cyst, thus irritated, falls into a state of inflammation, the effects of which are traceable through a variety of stages, to the almost total disappearance of both cyst and hydatid. The cyst first becomes thickened; lymph is thrown out on its internal surface, giving it a roughened granular appearance. The hydatid becomes opake, and its fluid contents muddy. An adhesion, probably of a glutinous or mechanical nature, is established between the lymph and the exterior surface of the hydatid. The fluid of the hydatid is then absorbed, and its empty bag squeezed up in the centre of the solidified tumour. At a period somewhat later all traces of the hydatid disappear, and the remaining mass consists of nothing but the altered cyst, filled with lymph and some curdy matter. The tumour diminishes in bulk, it becomes of a cheesy consistence, and finally is converted into a small solid nucleus of earthy matter, devoid, as it would appear, of any irritating properties.

Dr. Houston differs from other writers in referring the whole of the morbid changes to the cyst, and not to the contained hydatid, which he says is absorbed in the progress of the phænomena consequent upon its death. He does not concur with those who are of opinion that malignant and tubercular diseases are of the nature of parasitical animals. No facts have hitherto been advanced sufficient to establish the position that any such diseases are, either at their commencement or at any subsequent period of their progress, of such a character. No animal has ever been seen of any definite shape in connexion with them; and where the powers of the microscope can be brought with such effect in aid of investigations of this nature, why, it may be asked, if such pestiferous animals exist, have they not ere this been demonstrated? The fact is, that all the circumstances connected with the growth and decay of such parasites as our senses can take cognisance of tend to a conclusion of an opposite nature, *viz.* that these animals have their periods of existence as living beings, and having passed from this state, instead of polluting the whole frame, or running into extensive diseases, disappear, and leave little or no injurious effects behind them, unless what may have arisen from their mechanical interference with the functions of some vital organ.

The author exhibited numerous preparations and drawings illustrative of the facts advanced in the paper.

### *On the Entozoa which are occasionally found in the Muscles of the Human Subject. By Professor* HARRISON.

The Professor exhibited preparations and drawings of a speckled appearance not unfrequently met with in different parts of the muscular system, and detailed the particulars of several cases in which it had existed: he expressed his full concurrence with the opinions advanced by Mr. Owen, in the Transactions of the Zoological Society

of London, as to the animal or vital character of the bodies to which the appearance is owing. He next remarked some interesting coincidences in the cases he had examined: thus, in one instance, where the muscles were very generally affected, he found a large cyst in the liver which contained several hydatids. These were exhibited to the meeting. In all the other cases there were marks of scrofulous disease having existed, either recently or at some remote period: thus, in three cases the lungs were a mass of tubercular matter, and in another there was caries of the lumbar vertebræ and scrofulous suppuration in the adjacent structures. The Professor further stated, that in all the cases he had examined, this appearance was almost confined to the voluntary muscles: he had never met with it in the heart or intestinal tunics, but had found it about the circumference only of the diaphragm, and in the other mixed muscles to a much less degree than in the voluntary: these bodies he stated to be more numerous on the cutaneous than on the deep surfaces of muscles, and to be deposited in the interfascicular cellular tissue, rather than in the fasciculi themselves.

*On the Bones which are found in the Hearts of certain Ruminant Animals. By Professor* HARRISON.

The author first compared the circulating organs in fish, reptiles, birds, and mammalia. He next adverted to the opinions of Morgagni, Haller, Daubenton, Meckel, and Carus as to the singular osseous appendages which the hearts of some of the Ruminants possess, as also of some other animals allied to them. He exhibited several specimens of these bones, some dried, some in their recent state, and others *in situ*, in different animals. The heart of the ox presents them in greatest perfection; here there are always at least two, and sometimes several smaller osseous and cartilaginous grains: the two principal bones are, one very large, placed posteriorly in the septum auricularum; the other, smaller, is situated in front. The large one is of the figure of the human malar bone; its upper concave border forms the floor to the posterior aortic sinus; its inferior bevelled edge gives attachment to the large portion of the mitral valve; to the body of the bone the fleshy and tendinous fibres of the auricles are attached. The small or anterior bone is triangular; its concave base floors the anterior aortic sinus. These bones are always to be found in both sexes, and in the young as well as in the old. Specimens were preserved from animals only a few weeks old, in which osseous nuclei were distinct in the cartilaginous basis. The author next adverted to the peculiar fleshy character of the left ventricle in the ox, a transverse section exhibiting the appearance of a puncture or stab, rather than of a distinct chamber; this formed a curious contrast to the heart of the horse. From these and many other observations, Mr. Harrison inferred that these bones are supports, not only to this mass of muscle, but also to the root of the aorta which is connected to them, and which is thus maintained in a permanently

open state; while, again, there being two bones connected by ligament, the elasticity of the vessel is not impaired. These bones, moreover, serve to support the septum of the auricles, and to prevent their perfect closure or collapse, and they also floor and support two of the aortic sinuses with their semilunar valves. The Professor next spoke of the peculiar, hard, marble-like fat which is deposited in masses about the roots of the great arteries, and showed that these cover the three sinuses of the pulmonary artery, and that sinus of the aorta which is deprived of osseous support. These arterial sinuses are lodged in excavations in the fatty deposits alluded to, and no ordinary force can overcome the resistance which they offer to over-distension; and thus the sinuses are enabled to support the returning columns of blood, which are impelled by the elasticity or resiliency of the arteries, which in such animals are peculiarly strong and elastic. The author next explained the structure and true use of the corpora Arantii; contrasted the structure of the pulmonary artery and its valves with the corresponding parts of the aorta; and concluded with some observations on the calcareous and osseous deposits which are met with in the human subject, in whom they appear as accidental or morbid changes in those very situations where in some animals the osseous structure is essential.

*On the Structure of the Mammary Glands in the Cetacea; with Observations on the Mechanism of the Mouth and Soft Palate, as applied by the young Animal in Sucking. By* A. Jacob, *M.D., Professor of Anatomy, Royal College of Surgeons, Ireland.*

The author, commenting on the opinions of M. Geoffroy St. Hilaire in his work entitled "Fragmens sur la Structure et les Usages des Glandes mammaires des Cetacés," and referring to the descriptions of Hunter and plates of Müller, entered into the question of the mechanism of the mammary glands in Cetacea, and the operation of the mouth of the young of that tribe.

M. St. Hilaire is stated by the author to entertain the opinion that "the process of nutrition of the young of the Cetacea by the milk of the mother, is accomplished in a manner and under circumstances different from those of other mammalia." To support this proposition, M. St. Hilaire assumes that the mammary glands in these animals are peculiarly organized and circumstanced; first, in being placed between the abdominal and subcutaneous muscles, by which they are subjected to mechanical pressure adequate to the expulsion of their contents; and secondly, in containing a peculiar reservoir, formed by an enlargement of the excretory duct, and running the whole length of the organ.

These statements are admitted by Dr. Jacob; but he remarks, that there is no proof of any *special pressure* on the mammary gland arising from its position with reference to the muscles; and that the only *peculiarity* in the excretory ducts is the existence of the mammary reservoir, in the form of a single cavity, a circumstance which

the author considers to be dependent on the flat, elongated form of the mammary gland. He advances arguments to show the probability of there being, in fact, a special structure at the orifice of the nipple to *prevent* loss of milk by any other external pressure than that upon the teat or nipple itself.

Both M. St. Hilaire and Mr. Hunter have assumed that, in consequence of the opposite condition of the nostrils of the mother and young during the act of suction, this process can only be performed by the young between two respirations. The act of sucking, Mr. Hunter states, must also be different in the Cetacea from that of land animals, "the lungs having, in the former, no connexion with the mouth." On these points the author differs from the eminent authorities quoted, and enters into an examination of the action of the soft palate in the functions of breathing and deglutition; from which he deduces the conclusion, that *the mouth is a separate and distinct cavity, capable of increasing or diminishing its capacity, and, consequently, of forming an imperfect vacuum, into which the milk rushes in sucking, and from which, when accumulated, it is transferred to the œsophagus.* It must not be forgotten that the construction of the soft palate in the Cetacea is different from that in other animals: it is in them in the shape of a muscular partition, with a circular aperture surrounded by a sphincter; while the top of the larynx is elongated so much upwards that it enters this aperture, and, being grasped by the sphincter, communicates with the blow-hole or nostril, leaving the mouth and fauces unaffected by the process of respiration, and still better adapted than in other animals to carry on the operation of sucking.

### *On the Mechanism of* Bruit de Soufflet. *By Dr.* CORRIGAN.

The first part of the paper consisted of an analysis of the various theories which had been proposed to account for this sound and its varieties, *bruit de rape*, &c. Laennec supposed it to be produced by spasmodic action, but his opinion has been generally abandoned. By some the sound has been attributed to increased pressure made by narrowing of the heart or arteries,—but it is heard in permanent patency of the aorta, in the vessels of the pregnant uterus, in aneurismal dilatation of arteries in varicose tumours, in all which instances there is no narrowing;—by others to increased velocity in the motion of the blood; but it is not heard in the circulation of the fœtus or infant, while it is audible in the slower circulation of the mother; nor in the quickened pulse of hectic or inflammatory fever, while it is audible with a pulse of 70. By others it is attributed to roughnesses in the interior of arteries, or irregularities, over which the blood, in passing, produces the sound; but it is not heard in the healthy heart, the internal surface of which is exceedingly irregular; nor is it necessarily present in aneurisms rough and irregular on their inner surface, from shape, or from deposition of fibrine; the sound, on the contrary, being frequently heard when there is no de-

viation from the natural state of the interior surfaces of the heart or arteries.

The second part of the paper developed Dr. Corrigan's views. His theory is, that the sound depends on the simultaneous presence of these two conditions, viz. 1st, a current-like motion of the blood (instead of its natural equable movement), tending to produce corresponding vibrations on the sides of the cavities or arteries through which it is moving; and, 2ndly, a state of the arteries or cavities themselves by which, instead of being kept in a state of tense approximation on their contained inelastic blood (which would necessarily prevent any vibration of their sides), they become free to vibrate to the play of the currents within on their parietes; and by those vibrations cause, on the sense of touch, "*fremissement*," and on the sense of hearing, "*bruit de soufflet*." It was shown that these two conditions are present in the parietes of the ventricle, and the currents of blood striking against them in cases of narrowed auriculo-ventricular openings; in the enlarged and tortuous arteries of the placental portion of the uterus permitted by their very free anastomosis with veins and sinuses, and other causes, to become partially flaccid in the intervals of the heart's contractions, and the irregular currents necessarily assumed by the blood in rushing along these comparatively flaccid tubes at their next diastole; and that similar conditions exist in the analogous state of the vessels in aneurismal dilatations of tortuous arteries. The presence of the two conditions was also applied to explain the mechanism of the sound in permanent patency of the mouth of the aorta, in the large arteries of animals dying of hæmorrhage, and in various other instances. In conclusion, two experiments were detailed, in which, in one instance, a small bladder, and in the other a portion of the gut of an animal, was interposed between two cocks, the upper or nearer being the cock of a water-cistern, and the lower or further constituting the discharging orifice of the bladder or gut, and water then allowed to flow through from the cistern. The sound "*bruit de soufflet*," and the sensation "*fremissement*," were perceptible in the intervening bladder or gut, until (from the upper pipe pouring in fluid faster than the lower discharged it) the bladder or gut became tense, and then both sensations ceased, the passage of the fluid through, nevertheless, continuing all the time. The experiment with the bladder was applied to explain the occasional presence and absence of "*bruit de soufflet*" in aneurisms, the sound being present in an aneurism when, from any circumstance connected with it, its parietes can become at all flaccid in the intervals of the heart's contractions,—not being heard if the parietes remain tensely applied to their contained fluid.

Dr. Corrigan has in some experiments substituted a gum-elastic tube for the portion of gut.

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Dr. ALISON read a notice of a few experiments and observations which he had made, with the assistance of different friends, on two

distinct subjects: 1. On the Condition and the vital Powers in Arteries leading to inflamed parts, (in continuation of those on the same subject read to the Section in 1834); and 2. On the immediate Cause of Death in Asphyxia.

He connected them with one another by some preliminary observations on the importance of establishing the truth, and, as far as possible, determining the applications of the principle to which the term *spontaneity of movement* in the fluids of living bodies has been applied, *i. e.* of movements of the fluids in living bodies, which are dependent on their living state, but independent of any contraction of their living solids.

In proof of the truth of this principle he stated that many facts might be adduced; and the immediate object of the statements now made was to prove that without reference to this principle it is impossible to explain two sets of phænomena, which have been carefully observed, and are of essential importance,—the changes in the motion of the blood which attend inflammation, and those which result from the application of oxygen to the blood in respiration.

On the first point, he detailed the result of two examinations (in addition to those formerly reported) of the arteries of limbs of horses killed on account of injury and inflammation of single joints, in one case of three weeks', in the other of eight days' standing. The power of contracting on a distending force, and expelling their contents, was tried in the arteries both of the inflamed and the sound limbs, by the same contrivance as was used by Porseuille to compare the contractile power of living and dead arteries; *i. e.* by using bent tubes and stopcocks in such a way as to distend a given portion of artery (first of the one limb and then of the other,) by water pressed into it by a firm weight of mercury, and then allowing the artery to expel the distending water, and getting a measure of the force which it exerts in doing so, by the rise of the level of water in a tube communicating with the artery. The result was in both cases in accordance with the observations formerly made, that the artery of the inflamed limb exerted *less* power of contracting on, and expelling its contents, than that of the sound limb. The difference was as 10 to 16 in one case, and as 125 to 175 in the other, which was the more satisfactory of the two, as the experiment was made more immediately after death.

It appeared also, on careful comparative examination, that the contraction of the emptied arteries at the moment of death (which is the measure adopted by Parry of the vital power of arteries) was less in the diseased than in the sound limbs; the difference between the contracted state immediately after death, and the subsequently dilated and dead state of the artery (28 hours after death), being $\frac{1}{4}$th in the case of the diseased limb, and $\frac{1}{3}$rd in that of the sound limb.

It appears, therefore, that in all arteries of such size as to admit of measurement, and which supply inflamed parts, the only vital powers of contraction, which experiments authorize our ascribing to the coats of these vessels, is *diminished* during inflammation; and it

may be safely added, that no other change but this diminution or relaxation of contractile power has ever been perceived, either in them, or in the smaller vessels which come under the observation of the microscope, at least during the greater part, and in the highest intensity, of inflammation.

But if it be inferred from these facts that inflammation consists merely in relaxation of vessels, giving an increased effect to the impulse of blood from the heart to the part affected, several facts may be stated to show that the explanation thus afforded is quite inadequate. The change which takes place on the movement of the blood flowing to an inflamed part is, diminution of velocity or absolute stagnation in the vessels most affected, combined with increased velocity and increased transmission in all the neighbouring vessels; and it seems impossible to ascribe both these opposite effects to the same cause, viz. a simple relaxation or loss of power in the vessels concerned. Neither can the characteristic effusions consequent on inflammation, and by which alone it is uniformly distinguishable from simple congestion or serous effusion, (and particularly the increased quantity and increased aggregation of the fibrin that exudes from inflamed vessels,) be explained by this change of the action of the vessels. And further, the local causes which excite inflammation are not only such as in other instances produce an increase, instead of a diminution, of vital power, but they are such as have been ascertained to produce, when they are made to act on minute portions of individual vessels only, contraction instead of relaxation; as has appeared in the experiments of Verschuir, Thomson, Hastings, Wedmeyer, and others.

The proper inference, therefore, appears to be, that the idea of an *increased action of vessels* in an inflamed part is indeed a delusion; but that there is a really *increased action within the vessels* of the part, *i.e.* an increased exertion of powers, by which the motion of the blood is affected, but the action of which is independent of the contractions of the living solids, and the effect of which is to cause distention and relaxation of the vessels, within which they act with unusual energy.

2. The immediate object of the experiments on death by asphyxia was to ascertain whether the acceleration of the flowing blood through the lungs,—which is undoubtedly produced by respiration, and the failure of which appears, from the experiments of Williams of Liverpool, and of Kay of Manchester, to be the immediate cause of death by asphyxia, can be ascribed, as Haller and some very recent authors have supposed, to the merely mechanical influence of the alternate expansion and contraction of the lungs by the respiratory movements.

That this is not the fact might be concluded from the fatal asphyxia produced by breathing azote or other gases, not poisonous, but not containing oxygen; in which case it had been observed by Broughton and others, that the stagnation of blood in the lungs, and the distention of the right side of the heart, takes place equally as

when the respiratory movements are suspended. But to this observation it might be objected, that the animals on which experiment had been made had been allowed to remain in the azote until they became insensible, and their respiration of course ceased, and had not been examined until some minutes after their apparent death, and it might be said, that the right side of the heart had become congested only after the acts of respiration had ceased, and in consequence of their cessation.

In order to avoid this source of fallacy several rabbits were confined in azote, only until their breathing became laboured, the respirations generally less frequent, but much longer and fuller than natural. They were then taken out and instantly struck on the head with such force as to crush the brain and cerebellum, and arrest the circulation as instantaneously as possible. This was always attended with violent and general convulsion, but with no attempt at respiration, sensation being apparently instantaneously suppressed. When the body was opened immediately after the convulsion had subsided, the right side of the heart was always found distended with blood, and palpitating feebly; the left side at rest and comparatively empty: the quantity of blood obtained by puncturing and pressing the right side and pulmonary artery was from 5 to 10 times as much as could be obtained from the left side and aorta. When a rabbit previously breathing naturally was killed in the same manner, the quantity of blood on the right side of the heart (apparently accumulating there during the convulsions) was found to be greater than on the left; but the difference was decidedly less than when it had been breathing azote; and in one of these comparative trials the blood in the left side was found to be sufficient to keep up a feeble palpitation in that side, whereas in the animals that had breathed azote the left side was always found quite at rest.

It appears from these experiments that when oxygen is not admitted into the lungs in inspiration, even although the respiratory movements continued further and more forcible than usual up to the moment of death, the blood stagnates on the right side of the heart; and that the application of oxygen to the blood at the lungs is a cause of acceleration of its movement through the lungs, independently of any influence of the mechanical movements of respiration.

If we further inquire, in what manner oxygen can give this stimulus to the flowing blood through the lungs, it appears certain that it cannot be by stimulating the small capillaries of the lungs (the only vessels to which it is directly applied) to contraction, because even if it be granted that there are vessels capable of contracting on irritation (which is very doubtful), the immediate effect of stimulating any arteries capable of taking on such action has always been observed to be a constriction permanent for some length of time, and in consequence a *retarded* flow of the fluids through them, as in the experiments of Wedmeyer.

If, again, we suppose the effect of the oxygen on the minute ves-

sels in the lungs to be sedative or relaxing, and ascribe to a diminished action of these vessels the apparently increased efficiency of the right side of the heart when oxygen is applied, we suppose the oxygen to produce the very opposite effect to that which has always been observed when it or any other stimulus has taken effect on any individual artery.

The only mode in which it appears possible to escape from these difficulties is to suppose that the stimulus given by the oxygen to the flowing blood through the lungs, is a stimulus to that movement which is independent of any contraction of the solids containing the blood. This conclusion is in perfect accordance with the observations of Haller on the *derivation* of blood, perceptible under the microscope, towards any part where an opening is made in a vessel, and air admitted into contact with the blood, because he gives satisfactory reasons for thinking that this derivation is not owing to contraction of the vessels; it is also in accordance with observations on some of the lowest tribes of animals, and on vegetables, where *currents in fluids* are observed in connexion with the act of respiration, but no movement of solids has been detected; and even, as Dr. Alison thinks, with the observations of Purkinje and others, on currents connected with the respiratory organs in animals much higher in the scale, because although these last currents have been ascribed by most authors to vibrations of ciliæ, which are seen to accompany them in various instances, it seems very doubtful whether they can be adequately explained without supposing a "*jeu d'attraction et repulsion*" to be commenced in these instances, as well as in the respiration of the lowest tribes.

*Experimental Inquiry into the different Offices of the Lacteals, Lymphatics, and Veins in the Function of Absorption. By* P. D. HANDYSIDE, *M.D.*

The author's general position is thus stated: the lacteals, lymphatics, and veins are endowed each with a peculiar office in the general functions of absorption; for example, 1. The *lacteals* are those vessels which absorb the aliment which is necessary for maintaining the nutrition and increase of the body, and exercise the property of refusing entrance to all other matters; 2. The *lymphatics* absorb the elements of the body upon their becoming useless or noxious, so as by their final discharge from the system to make room for the deposition of new matter, and these vessels possess no absorbing power over any substances foreign to the system; 3. The *veins* not only return to the heart the blood after that fluid has fulfilled the object of its diffusion over the system, but enjoy the office of receiving into the animal system by absorption various *foreign* matters which may be brought into contact with their orifices.

In support of these views the author presents a short review of results obtained by various eminent anatomists and physiologists.

The following is the order of the subjects discussed:

*Lacteals.*—Their distention after a full meal,—their condition as observed in living animals;—effects of ligatures on the thoracic ducts of horses.

*Lymphatics*,—Anatomical origin of,—analogy of lymphatics and lacteals,—exact resemblance of the lymph prior to its absorption to that found in the lymphatic vessels,—absence of lymphatics in vegetables,—no proof afforded by examination of lymph that lymphatics serve as the channel through which foreign matters gain entrance into the system,—no communication between lymphatics and veins except through the great lymphatic trunks.

*Veins.*—Analogy between the anatomy and disposition of the veins of animals and the vessels corresponding to these in plants, favours the doctrine of *venous absorption.*

"When foreign matters capable of affecting the constitution generally, and of being diluted in its solids and fluids, are brought into contact with the *serous and mucous surfaces* of the body, with the *cutis vera*, and with the interstitial cellular tissue of different organs, the resulting phænomena exhibited by the blood in the veins give evidence that these vessels are the sole agents employed in this variety of absorption." These four points are discussed by reference to a variety of experiments, to which the author adds the following from his own researches, as bearing on the question of absorption of *foreign* matters by veins of the *cellular tissue.*

Exp. 1. Having made a fistulous opening in the abdominal parietes of a *dog*, he took advantage of the period when a complete granulating surface should be formed, to apply to it very freely the solution of pruss. potass. On killing the animal three minutes after the application, and applying the appropriate chemical test to the blood, it was seen to exhibit traces of the prussiate.

Exp. 2. He induced the formation of a granulating surface four inches square in extent in the fleshy substance of the back of a large *cat*, and then retained pledgets of lint moistened with 1 ℥ of the usual solution of the prussiate of potash in contact with this surface during the space of four hours. A fair indication of the presence of the poison in the blood was seen, on submitting to the usual test the blood from the carotid arteries, both in its fluid and coagulated states, while no indication whatever of its presence was observed in the lymph.

These experiments now put forth as evidence in favour of the doctrine of absorption by the veins of *foreign* matters, from the ***interstitial cellular tissue*** of the animal body, when taken along with the previous experiments also adduced to prove the absorption of ***foreign*** matters from the surface of the ***cutis vera*** and the different ***mucous*** and ***serous*** superficies, would appear to justify a ***conclusion***—that the absorption of *foreign* matters occurring from the interstices and surfaces of the body occurs solely through the channel of the venous system.

## *Observations on the Effects of Cold on different Parts of the Human Body, and on a Mode of measuring Refrigeration.* By *Dr.* Osborne.

In this communication Dr. Osborne began by adducing some facts to show the importance of cold, viewed as a cause of disease. He stated, that of 57, the entire number of patients on the preceding day (13th August, 1835,) in Sir Patrick Dun's Clinical Hospital, 34 could distinctly refer to cold as the cause of their complaints, contracted in the following manner: in 12 from damp clothes, 5 from damp feet, 3 from bathing, and 14 from cold air when heated. This proportion, however, would probably be very different in winter. The direct effect of cold on the air-passages of the lungs is almost restricted to inflammation at the rima of the glottis, and this is usually caused by suddenly rushing from heated to cold air. It may be proved that the respired air, being of nearly the same temperature as the blood, and not deriving its heat from the action of respiration in the lung (see Brodie's Experiments), must, in its passage downwards, be heated to considerably more than half the difference between the temperature of the blood and that of the air; that, consequently, at its arrival in the air-vesicles of the lungs, it must have acquired such a temperature as amounts to a protection against the effects of cold. Dr. Osborne considers this as a provision of nature in a matter in which we are not able to guard ourselves.

When, owing to an oppression of nervous energy, the healthy temperature of the surface is not maintained, then the air arrives at the air-vesicles without being heated; hence, he conceives, may be explained the numerous instances of sudden death which occur in chronic bronchitis and low fevers when sudden depressions of the temperature of the atmosphere have taken place during the night. In those cases the cold thus admitted to the lungs causes a torpor in their capillary circulation; and after death it is found that the blood has stagnated in the lungs, and in the veins and right cavities of the heart.

The common opinion that various inflammatory diseases are contracted by sleeping in newly-built houses appears to be ill founded, except in as far as the clothes worn by the individual may contract moisture. The air under the bedclothes being kept up by the heat of the body to the temperature 80°, the only way in which the damp air can prove injurious is by the lungs, which, as before stated, are, in health, enabled to resist its effects. It appears that in a regiment which was quartered in newly-built barracks no injury resulted from the damp.

On the stomach the effect of cold is perceived, not by a sensation of cold in that organ, but by thirst, in consequence of reaction, as is experienced after taking ices. When the cold is long-continued or overpowering, in consequence of feeble reaction, then gastritis is produced from torpor of the capillaries. This last mode of explanation is derived from the phænomena observed in the exte[illegible] of the

body on the application of cold. When the application is transient and the circulation vigorous, the contraction of the vessels and paleness of the surface are only momentary, and are succeeded by reaction evinced in increased heat and diffused blush of redness. When it is long continued, then the pale and shrunk state of the surface is gradually succeeded by a purple or livid colour, attended with increase of size, as may be proved by a ring on the finger, from the swollen state of the vessels. Comparing these facts with the experiments detailed by Dr. Alison,—which showed that in inflamed parts not only the small vessels but the large arterial trunks leading to the part are dilated, and rendered incapable of contracting like other arteries,—Dr. Osborne proposes the question, whether there is not sufficient evidence to prove that cold produces inflammation by producing torpor and dilatation of the vessels, either of the part itself or of some connected or adjacent part, which, if not removed by transient reaction, is followed by the more permanent reaction of inflammation, causing a number of new phænomena.

With regard to the effect of cold on the skin, which is the most important of all, it is evident that meteorology has contributed very little to our knowledge of the influences of the atmosphere on health or disease. It has appeared to the Author, that in order to connect this science with utility, as far as mankind is concerned, one consideration has been omitted, which is, *the cooling power of the atmosphere estimated with reference to ourselves.* The human body has a heat of nearly 98°, and is placed in a medium always cooler than itself. The degree of cooling influence exerted on it has never been made the subject of measurement, and to the present time is estimated solely by the feelings. In order to measure the cooling influences of the air or other media, Dr. Osborne used a spirit thermometer, without a frame, carefully graduated from the degree 90 to 80 inclusive, that being nearly the temperature of the exterior of the body. Having heated the bulb to 90°, he exposed it in different situations, observing the time during which the spirit descended from 90° to 80°, and adopting, as a measure of the refrigerating power, the rate of cooling deduced. And by this contrivance is exhibited the result of radiation, and of the conducting power of the atmosphere as modified by its temperature, its density, its moisture, and its currents; and that result, the most interesting of all to the invalid, who, in respect to temperature, may be conceived as represented by the instrument. As the variety in the shape of the bulb, the bore of the tube, the thickness of the glass, or the density and quantity of the fluid employed will cause variety in the time of the descent, the result obtained with two thermometers must not be expected exactly to correspond. In order to procure uniformity for this purpose, it will be necessary to place a number of them, previously graduated between 90° and 80° and heated to 90°, in air at 60° or 50°, and to select those which contract according to the time fixed on as a standard. The thermometer so applied, Dr. Osborne proposes to call a psychometer, or measurer of refrigeration.

Amongst the observations brought forward by him to illustrate its use are the following:

To show the refrigerating effect of agitation or of a breeze, the temperature of the air remaining the same.

In air, temp. 70° at rest, it cooled from 90° to 80° in 5$^m$ 20$^s$.
——————— in a slight breeze.................... in 2$^m$ 50$^s$.
——————— blown on with a bellows........... in 58$^s$.

These observations show the fallacy of determining climate by the thermometer. There are situations in which, owing to constant currents of air, a cold is produced of the utmost consequence to health, but not appreciable by the thermometer. Dr. Osborne expects that by means of this mode of observation much light may be thrown on the climates of the western coast of Africa, and of other unhealthy localities. The meteorological tables at present kept in those places fail in showing the effect of the sea and land breezes.

The following shows the refrigerating power of water above air of the same temperature, at rest, to be above 14 to 1.

In air at rest, temperature 70°, it cooled from 90° to 80° in 5$^m$ 40$^s$.
In water at rest, same temperature.......................... in 24$^s$.

It is well known that in swimming it is not the fatigue so much as the refrigeration which fixes the limit. This appears from the following observation compared with the preceding.

The instrument agitated in water, cooled from 90° to 80° in 15$^s$.

In order to ascertain the refrigeration produced by damp clothes, Dr. Osborne covered the bulb of the instrument with cotton wool, and having placed it at rest in an apartment at 68½°, found it to cool from 90° to 80° in 10$^m$ 14$^s$. Placing it in the same circumstances, but with the cotton wool slightly damped, it cooled down in 2$^m$ 57$^s$. This proportion must be much increased when under the influence of the open air. The application of cotton wool to the skin, moistened with water or an evaporating lotion, he has found the most eligible means of cooling the surface in disease, not only on account of the constancy with which the refrigeration is maintained, but from its being peculiarly agreeable to the feelings of the patient.

*On the Influence of the Artificial Rarefaction or Diminution of Atmospheric Pressure in some Diseases, and the Effects of its Condensation or increased Elasticity in others. By Sir* James Murray.

The paper was divided into two parts. The first detailed the general principles of the *rarefaction* of air, and its powers as a remedial agent on the human body. The second part related to the local agency of condensation of air in topical diseases.

The propositions were submitted, not as remedial means of themselves alone, but as auxiliary to those already in use. It was shown,

That the ordinary atmospheric pressure sustained by the whole body averages 15 tons;—that by placing a person in an air-tight bath, with provision for breathing the ordinary atmosphere, half a ton or a ton can be removed without danger:

That the abstraction of this elastic compression permits the easier expansion of the chest, elicits the blood and animal heat to the surface of the body, *opens the pores of the skin, and restores to the surface* rashes or eruptions which had been suppressed.

It was therefore submitted, that an agent capable of producing such effects is entitled to consideration in treating certain conditions of pectoral diseases; in eliciting internal congestions or inflammations from central organs to the surface; in preventing certain fevers, and other complaints arising from obstructions of the cutaneous functions; in translating gout and rheumatism from vital organs to the limbs; in restoring a due balance of the circulation, and attracting the blood into the superficial veins from the deep-seated arteries.

A case of a patient was detailed, in which congestion of the brain was diverted from the head by inclosing one of the lower extremities in a rarefying bath, and abstracting about two pounds and a half of pressure from each inch of the surface: the influx of the fluids was so great, that in two hours the circumference of the limb was increased nearly three inches, the vessels of the skin rendered red, warm, and turgid, and the head relieved.

The case of a painter was also adduced, whose right arm had long been paralysed and cold from the effects of lead paint. The arm was put for two hours into the rarefying case, and afterwards continued hot and vigorous, so that the man was able to resume his work.

*Part second.*—As diseases of an opposite nature require opposite remedies, the principle of *rarefaction* is *reversed* in certain cases, and *condensation*, or additional pressure, employed.

This part of the paper detailed several cases illustrative of the powers of this agent. Where there was too much vascularity of parts, then local pressure, pumped under an air-tight covering, emptied the vessels, propelling onwards the overflow of blood contained in the veins, and preventing its undue influx by the arteries.

The consequences were, to diminish inflammations, dissipate tumours and white swellings, facilitate the reduction of hernia and other protrusions, and to diminish the influx of fluids into indurated breasts or enlarged glands.

The author adduced a very interesting case, the reduction of a *prolapsus ani* by atmospheric pressure, without touching or bruising the sensitive intestine.

The powers of condensation of air were then alluded to, for the treatment of fungous sores or ulcers, and for the suppression of uterine hæmorrhages, as well as bleeding from wounds or lacerations.

## *On the Differential Pulse. By Dr.* M'DONNELL.

Dr. M'Donnell's paper began with a description of what he terms "the Differential Pulse," and with proofs of his claim to priority in ascertaining it in 1784. The observations which succeed related to the following subjects.

The influence of disease and of particular remedies upon the pulse, with a reference to the effect of posture on the number of beats; the absence of this phænomenon in quadrupeds, owing to their natural vessels being horizontal in both the lying and standing posture; certain cases of health and disease, in which the maximum and minimum of this variation are found; the methods to be pursued for investigating the number of the pulse in wild and ferocious animals as deducible from their respirations; the proportion between the stops, pulses, and respirations in man and quadrupeds in active exercise; observations made at a depth of 26 feet in a diving-bell, which corroborate the views of Sir David Barry and Dr. Carson on the moving powers in the circulation; proofs that barometrical variations have no influence upon the pulse or breathings.

Part 2.—On the limitations of the doctrine of the "Differential Pulse"; of stationary or permanent pulses; observations made on the pulses of children before and after their having respired; of the acceleration of the pulse after birth; observations on quadrupeds with respect to this; supposition that the fœtus remains before birth in the state of the cold-blooded animals; of the final cause of this peculiarity; of the cause of the stethoscopic sounds of the fœtal heart being very rapid, although the pulse in the funis be slow; an account of an experiment made by a watch ticking under water; of the remarkable strength of the fœtal pulse as felt in the chord; of the absorption of the blood in the chord into the system of the fœtus after delivery; and the inference from this in favour of the views of Sir David Barry and Dr. Carson respecting the suction power of the thorax as influencing the circulation.

## *On some hitherto unobserved Differences in the Effects of Accumulations of Liquids or of Air within the Cavity of the Thorax. By Dr.* WILLIAM STOKES.

In this communication Dr. Stokes pointed out a new source of diagnosis, namely, the paralysis of certain of the respiratory muscles which results from their vicinity to an inflamed tissue.

The excitement of muscular tissue produces at first active contractions, as shown by pain and spasm; but when still longer continued, the contractility is either lost or greatly diminished. These effects may be produced in a threefold manner: 1st, by direct irritation of the muscular tissue; 2ndly, by the contiguity of muscle to an inflamed tissue; and 3rdly, by irritation in the cerebro-spinal centres or nerves. Of these, the first has been recognised in certain thoracic diseases; but the second, or the paralysing effect, has been hitherto neglected.

There are three diseases of the thoracic viscera in which this injury of muscular action takes place *from the inflammation of a contiguous structure,* namely, bronchitis, pleuritis, and pericarditis, and in all there is evidence of this paralysis taking place.

The author dwelt principally on the lesions of the intercostals and diaphragm, and showed that in emphysema the characteristic smoothness of the side and depression of the diaphragm were to be attributed to this paralysis, resulting from the previous inflammation. He showed that it was commonly absent in Laennec's emphysema, even where the chest was much enlarged; that it did not occur in enlargements of the liver, and was often absent in hydrothorax. Dr. Stokes suggested that in bronchitis this paralysis affecting the circular muscles of Reissessen might be the cause of fatal accumulations in bad catarrhs, and further, that dilatation of the bowels might in some instances arise from it. He offered an explanation of death in pericarditis, by referring it to this paralysis affecting the heart, and suggested a new explanation of the succession of hypertrophy with dilatation to pericarditis, in which the yielding of the heart and cavities during its weakened state caused the dilatation, while the subsequent hypertrophy was due to the efforts made by the heart, on recovering its tone, to propel the blood into vessels not proportionally dilated.

Lastly, the stethoscopic phænomena of accumulation of air, as in Laennec's emphysema, were shown to be greatly modified by the yielding of the chest. The author came to the conclusion that the feebleness of respiration in this case is more an indication of compression of the lung than a direct sign of the muscular emphysema of Laennec.

### *On Aneurism by Anastomosis. By* R. ADAMS, *A.M., Member of the Royal College of Surgeons.*

The subjects treated of were divided into two sections. 1. On aneurism by anastomosis of the capillary arteries and veins. 2. On the pulsating form of the disease. The structure of these different forms of aneurism was described as far as is yet known, many cases being detailed and drawings referred to; a mode of investigating the ultimate arrangement of the minute arteries was recommended; and a short allusion made to the subjects of diagnosis, prognosis, and treatment of the disease.

### *Abstract of a Case of deficient Development of the right Hemisphere of the Brain, with Congenital Malformation of the Hip and and Atrophy of the Members of the same Side. By Dr.* HUTTON.

The subject of this observation was an idiot of adult age, and was only three or four days under the writer's notice when he died of an acute inflammat d

his observations to the instances of deficient and perverted development.

It appeared that the head was not deformed, but the brain was small, and the cranium preternaturally thick, particularly in the frontal region. A very considerable portion of the right hemisphere of the brain was deficient, and its place occupied by a large cyst containing limpid serum. There was also a remarkable deficiency of development in other parts of the cerebral mass at this side. The optic thalamus, corpus striatum, right pair of the tubercula quadrigemina, crus cerebri, and corpus pyramidale were all less developed than the corresponding parts of the opposite side. The optic and other nerves were normal, as were also the arteries at the base of the brain. The total absence of convolutions and of the grey substance of the brain in the situation of the cyst, the deficient development of the other parts enumerated, the healthy consistence and appearance of the cerebral mass and its membranes elsewhere, together with the history of the case, seemed to indicate that the phænomena were the result of original conformation, and not of any subsequent morbid action.

The left inferior extremity presented all the external characters of an accidental dislocation of the thigh upwards and outwards; but it appeared from an examination after death, as well as from the history of the individual, that the dislocation must have been congenital. The acetabulum was imperfectly formed, and not adapted to contain the head of the femur. This bone was also of abnormal form. The axis of the head and neck fell directly on the anterior instead of on the internal surface of the shaft, as if the bone had been twisted; the head of the femur was smaller and less spherical than usual, and the bone was in some degree atrophied. The ligaments were of healthy structure, and retained their usual connexions, but seemed to have been gradually elongated so as to allow the head of the bone to rest on the dorsum ilii. There was no cavity formed here, but some strong ligamentous bands connected the capsule, where it invested the head of the femur, with the surface of the ilium. The interarticular ligament was of a broad, taper-like form and much elongated; the cotyloid formed a flat moulding round the acetabulum. The muscles were healthy in structure, but in some degree atrophied, and the direction of their fibres was changed according to the altered relative situation of their usual lines and points of origin and insertion.

There was no trace whatever of previous violent injury or of disease of any of the structures in or around the articulation.

The left arm was flexed, atrophied, and nearly useless; the hand was extremely pronated. The flexed state of the limb seemed to depend on the contracted state of the muscles; the pronation of the hand on the circumstance of the inferior cubito-radial articulating surface of the ulna presenting inwards, instead of outwards and forwards, and the radius being accommodated to it.

The subject of these observations seemed to have very few ideas, and these were of the most simple kind, principally connected with

his sensations. He apprehended some, however, to which he could not give utterance. He had little use of language, and articulated indistinctly. He was reported to have imperfect vision in the left eye, and to have been lame from his birth; but he walked much, inclining forwards and to the left side, and touching the ground with the toes only of the left foot: he used no support. His left arm was nearly useless.

The apparent relation between the several portions of the brain in their development; the normal state of the optic nerves in connexion with the defective condition of the right pair of the tubercula quadrigemina; the similarity in the state of the arteries supplying either hemisphere of the brain; the imperfect and perverted development of the left extremities in connexion with the deficiency in the right hemisphere of the brain; and the anatomical condition of the left hip-joint, seem to the author to be the principal points of interest in the case which he detailed.

*Description of a Case of Deformity of the Pelvis, in which the Cæsarean Operation was successfully performed. By* G. B. Knowles, *M.R.C.S., F.L.S., Lecturer on Botany at the Birmingham School of Medicine.*

*Propositions concerning Typhus Fever, deduced from numerous Observations. By Dr.* Perry.

1st. That typhus fever is an idiopathic disease, solely produced by contagion, that is, by the introduction into the system of a specific animal poison.

2nd. That this specific poison is (as far as yet known) only generated in the human body during the course of this idiopathic fever.

3rd. That no other fever, arising either from general causes, as cold, fatigue, improper ingesta, local lesions, or marsh miasmata, is capable of generating this specific poison, or, in other words, producing contagious typhus.

4th. That this contagious idiopathic typhus runs a certain course, which may be modified, but cannot be checked, and is distinguishable from all other fevers by certain symptoms, which, in a greater or less degree, are uniformly present during its course.

5th. That the following is the usual course of the symptoms by which contagious typhus may be distinguished, viz. languor, nausea, frontal headach, rigors, loss of strength and appetite, followed by increase of thirst, quickened pulse, heat and dryness of skin, pain of back, or general soreness over body. The tongue becomes white at the base and centre, florid at the tip and edges, and on the fifth day from the first attack of headach, rigors, or nausea, a reddish slightly elevated, but irregular papular or measly eruption comes out, sometimes sparingly, at other times thickly scattered over the trunk and limbs, but rarely appearing on the face. As the fever advances in severity, the frontal headach abates, the tongue becomes dry and brown

in the centre, the eyes dull, heavy, and suffused, the pulse quicker, the thirst more urgent, the skin more dry and warm, and the mind disturbed. On the sixth day the eruption becomes more general and distinct, like rubeola, occasionally fading or disappearing suddenly, and becoming as the disease advances flattened and of a darker or duskier hue; and when the fever is accompanied with congestion of the brain or lungs, or by thickening of the lining membrane of the bronchial vessels, it assumes that livid appearance usually called petechia. This eruption, when slight, frequently disappears in a few days, but more frequently is visible during the whole course of the disease.

6th. That simple unmixed contagious typhus usually continues for fourteen days from the first attack, when the febrile symptoms abate, the eye becomes clearer, the skin softer, and the mind composed, with less thirst; there is sometimes a slight abatement of the symptoms on the tenth day, particularly in children, at times nearly complete.

7th. That when the febrile symptoms continue beyond the fifteenth day without abatement, local lesions exist, to which must be attributed the longer continuance of the febrile symptoms. The appetite frequently continues defective till the twenty-first day, when all the functions resume their healthy action.

8th. That contagious typhus is often to be met with in combination with other diseases, usually of a local character, as of the lungs, the mucous membrane of the stomach and intestines, more particularly the aggregated glands of the ilium, or the membranes of the brain.

9th. That by the local diseased action of the parts above mentioned, the febrile action of the system is kept up; but the character of the disease is changed; and in such cases it frequently goes on to a fatal termination, or abates upon the twenty-first day from the commencement of the disease.

10th. That between the ages of seven and fifty nineteen out of twenty are susceptible of being affected by contagious typhus if exposed to the contagion, and not protected by having previously had the disease; but that children under five years are rarely affected with contagious typhus, and under two may continue to suckle or sleep with the mother labouring under typhus without catching the disease.

11th. That contagious typhus is an exanthematous disease, and, like smallpox, measles, and scarlet-fever, during its course produces some change on the system, by which the individual having once undergone the disease, is (as a general rule) secured against a second attack, and may with impunity expose himself to the contagion of typhus, if he continue to reside in the same country in which he previously had the disease.

12th. That contagious typhus never exists in combination with any of the exanthematous diseases.

13th. That in every case of pure typhus the blood undergoes during the disease a considerable change, becoming darker in co-

lour, and in many cases losing the power of coagulating when drawn from the arm, and in all cases being more loose in texture; and when death ensues during the course of the disease, the blood contained in the heart and large vessels is dark and fluid.

14th. That inflammation of the membranes of the brain, of the bronchia, and of the mucous membrane of the stomach and intestines, and various febrile affections arising from cold, fatigue, improper ingesta, &c. &c., more particularly disease of the aggregated glands of the ilium, and the mucous follicles, usually termed dothenenterite, have been confounded by medical practitioners with typhus fever, and are characterized by dissimilar symptoms, and require a very different mode of treatment.

15th. That the congested state of the vessels of the brain, the serous fluid on its surface, and the dark fluid state of the blood are the most constant morbid appearances to be met with in post mortem inspections of those who have died of contagious typhus; and in many cases the only morbid appearances which are to be found, and the next in frequency, is the thickening or darker appearance of the lining of the bronchial membrane; and the third in order is the diseased state of the mucous membrane of the intestines, more particularly that of the aggregated glands of the ilium. The relative proportion of these states to each other varies considerably, according to the state of the weather as to heat, dryness, &c.

16th. That dothenenterite, or enlargement of the mucous follicles of the smaller intestines, and enlargement and ulceration of the aggregated glands of the lower third of the ilium, occurs in combination with contagious typhus, and is to be met with in about one in six of those who die from typhus, but that it also exists as a disease *per se*, where it is characterized by the following symptoms: A quick, irritated pulse; tongue dry and florid at the tip or throughout, often fissured in centre; thirst urgent; eyes clear; no complaint of frontal headach; face alternately flushed and pale, more particularly flushing of one or both cheeks; less decided anorezia; pain of epigastrium, or in the right and left iliac regions on pressure; occasional vomiting of a greenish fluid; stools in general natural; abdomen slightly tumid and puffy, but the patient makes little complaint. The disease may exist in every degree of mildness or severity, having no regular period of termination; it may run on for two, three, or even four weeks, and terminate in gradual restoration to health without any sensible crisis; or the patient may sink under it from exhaustion, or by hæmorrhage from the bowels; or it may end by some of the ulcers of the aggregated glands of the lower third of the ilium penetrating the coats of the intestines, and part of the contents being effused, exciting peritonitis, under which the patient sinks in the course of two, or at most three days.

The less compressible state of the pulse, the clearness of the eyes, the flushing of the cheeks, the more florid, parched, and fissured state of the tongue, the comparative absence of the frontal headach, and the complete absence of the typhus eruption, sufficiently distinguish this disease from contagious typhus. To a practised eye, the

colour of the face, the flushing of one or both cheeks, the clear eyes, with the irritated state of the pulse, are sufficient to point out the disease and distinguish it from typhus.

When in combination with typhus, all the symptoms are aggravated in severity after the fourteenth day, and then become more distinctly marked. This disease is an equally frequent accompaniment of smallpox as of typhus, and presents the same morbid appearances on post-mortem inspection.

In those who have died of this disease, of the mucous membrane, of the intestines, the blood in the heart and large vessels presents the same appearances as in those who have died of chronic inflammatory disease, in this respect differing from its appearance in typhus cases.

The observations on which these facts are founded were not made to establish any theory. The reports of the cases and post-mortem inspections are made in the presence of all who choose to witness them; the facts only are pointed out, and all are at liberty to draw their own conclusions from them.

These sixteen propositions, the result of careful observation in upwards of three thousand cases and two hundred post-mortem inspections, are considered as facts fully ascertained. The following are believed, but sufficient evidence cannot as yet be adduced to prove them.

1st. That typhus fever does not become infectious till the sixth day, and is most contagious when the patient is in the convalescent state, when cuticular desquamation usually occurs:

2nd. *That the contagious poison is chiefly spread by the disquamation of the cuticle during the period of his convalescence*:

3rd. That the earliest period of the disease making its appearance after exposure to contagion is eight days, more frequently fourteen, and sometimes as long as two months:

4th. That in every case of genuine typhus fever the vessels ramified upon the pia mater are more or less enlarged &c congested, and throw out a serous fluid betwixt the convolutions of the brain and on its surface, which to a certain degree compresses this organ and impairs its functions, and along with the morbid state of the blood is frequently the cause of death in this disease.

## *On the Use of Chloride of Soda in Fever. By* ROB. J. GRAVES, *M.D.*

Dr. Graves commenced a series of clinical experiments in 1832 upon the efficacy of chloride of soda in petechial and maculated fever. He has exhibited this medium at Sir Patrick Dun's Hospital and at the Meath Hospital, where its effects have been witnessed by a great number of physicians as well as pupils. The form recommended is Labarraque's solution, which is a saturated solution of chloride of soda. This was given in doses of from fifteen to twenty drops in an ounce of camphor mixture every hour. In the

commencement of fever, where there is great heat of skin and signs of vascular excitement, its employment is contraindicated. It is also inadmissible in cases where there is decided evidence of visceral inflammation. When the early stage of fever is past, when all general and local indications have been fulfilled, when there is no complication with local disease, when the patient lies sunk and prostrated, when restlessness, low delirium, and more or less derangement of sensibility is present, when the pulse is quick, when the body is covered with maculæ or petechiæ, and the secretions from the skin and mucous membranes give evident proof of what has been termed a putrescent state of the fluids, it is then that the chloride of soda may be prescribed with advantage. It operates, although not rapidly, yet energetically, in arresting many of those symptoms which create most alarm. It seems to counteract the tendency to tympanitis, to correct the fœtor of the excretions, to prevent collapse, to promote a return to a healthy state of the secretions of the skin, bowels, and kidneys; in fact, it appears admirably calculated to meet the bad effects of low putrid fever. Its employment does not preclude the use of wine or other approved remedies. Dr. Graves has used it in several hundred cases of typhus, and strongly recommends its employment in that disease.

### *Original Views of the Functions and Diseases of the Intestinal Canal, &c. By Dr.* O'Beirne.

Dr. O'Beirne commenced by stating, that although the great majority of published and private opinions are strongly in favour of the views on this subject put forth in his late work, many objections and prejudices remain to be removed; and that although his mode of treating enteric diseases has been most successfully employed for nearly twelve years in Ireland, and wherever it has been tried during the last two years and a half, it has not yet come into as general use as might be expected. He then addressed himself to the objections which have been urged against his theoretical and practical views, and advanced a great number of facts and arguments to show the unsoundness of those objections. Finally, he briefly related several cases of dysentery, strangulated hernia, and tympanitis, in which the new treatment proved superior to any other, for the purpose of removing the prejudices which appear to prevail so generally against its employment in those diseases.

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### *On Purulent Ophthalmia. By Dr.* Evory Kennedy.

Dr. Evory Kennedy gave a report of numerous cases of purulent ophthalmia of infants, in which leeching, constant removal of the purulent secretion and caustic applications, modified according to the violence of the attack and, in aggravated cases, the solid nitrate of silver, applied to the interior of the lids, had proved most successful.

A notice of the curved Drill Catheter, invented by Mr. FRANCIS L'ESTRANGE, was presented to the meeting.

Mr. HAWKINS exhibited to the Section specimens of Harrington's patent Electrizer.

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*Abstract of a Registry kept in the Lying-in Hospital of Dublin. By* ROBERT COLLINS, *M.D., late Master of that Institution.*

The numerous tables accompanying this communication were taken by Dr. Collins, with much care, from a registry kept by him of 16,414 deliveries occurring in the Dublin Lying-in Hospital in a period of seven years, commencing November 1826, during which he had the medical charge of the institution.

The tables are placed in the following order: first, tables relating to all preternatural presentations met with in 16,414 deliveries; next, those relating to labours complicated with hæmorrhage; retention of the placenta; convulsions; rupture of the uterus or vagina; with two or more children: or when the frenis umbilicalis descended before the child; and lastly, relating to the number of still-born children, and the number dying during the period of the mother's residence in the hospital.

The total number of preternatural presentations met with in the hospital during Dr. Collins's residence as master was 409, (not including those occurring in *twin cases*,) or 1 in every 40. Of the 409, 242 were breech presentations, 127 were cases of presentation of the feet, 40 of the arm or shoulder.

Of cases of hæmorrhage, 11 were unavoidable; 13 were accidental; 64 occurred between the birth of the child and the expulsion of the after-birth; 43 were subsequent to expulsion of the placenta.

Sixty-six cases of retention of the placenta occurred; 30 cases of convulsions; 34 cases of rupture of uterus or vagina; 240 women were delivered of twins. Of the 480 children, 422 were born alive; 245 were males; 309 presented naturally; 73 with the breech; 60 with the feet; 7 with the arm or shoulder.

There were 97 cases of prolapsus of the umbilical chord.

Of 16,654 children, 1,121 were still-born: 527 of these were putrid; 614 were males.

Of 16,654 children born, 214 died previous to the mother leaving the hospital (generally on the 8th or 9th day after delivery).

All the results above stated are further analysed with reference to the age of the mother; first, second, &c. stage of pregnancy; length of time in labour, and other circumstances important to medical science. It has been found impracticable to abstract the numerous and detailed tables and deductions contained in this valuable registry, so as to do justice to the author's views, without occupying many pages. Dr. Collins refers on some points to his 'Practical Treatise on Midwifery.'

## MECHANICAL SCIENCES APPLIED TO THE ARTS.

### *On Impact and Collision.* *By* EATON HODGKINSON.

Mr. Hodgkinson reported to the Section the results of certain experiments made by him on impact and collision, in continuation of those communicated to the Association in the year 1834 on the collision of imperfectly elastic bodies. The results were,

First, That cast-iron beams being impinged upon by certain heavy masses or balls of metal of different kinds, were deflected through the same distance, whatever were the metals used, provided that the weights of the masses were equal.

Secondly, That the impinging masses rebounded after the stroke through the same distances, whatever was the metal of which they were composed, provided that the weights were the same.

Thirdly, That the effect of the masses of different metals impinging upon an iron beam were entirely independent of their elasticities, and were the same as they would give if the impinging masses were inelastic.

Mr. Hodgkinson also gave the result of some interesting experiments on the fracture of wires under different states of tension, from which it appeared that the wire best resisted fracture and impact when it was under the tension of a weight which, being added to that impinging upon it, equalled one third of the force that was necessary to break it.

### *On the Solid of least Resistance.* *By* J. S. RUSSELL.

Mr. Russell was called upon to give an account of a new form for the construction of ships, by which they should experience least resistance from the water in their passage through it. A vessel of 75 feet keel and 6 feet beam had been built on this new formation, and made the subject of very accurate experiments, from which it appeared that this vessel, named the "Wave", experienced much less resistance in passing through the water than vessels of the very finest formation and from the best builders on the old construction.

Mr. Russell then detailed very minutely the mode of forming any vessel on this plan when the length and breadth were given. The peculiarity, in general terms, appears to be the formation of the entrance lines from parabolic arcs, so as to have a point of inflection at about one sixth part from the bow of the vessel, before which the bow is concave externally, giving the finest possible entrance at the stern, at an angle of contact infinitely small, and behind which the convexity is external, and the formation elliptical to the midship section, after which the formation becomes wholly ellipsoidal. Mr. Russell had been induced to consider this solid as the solid of *least* resistance from a phænomenon that appeared to distinguish this form from all

others, namely, that it entered the water at the highest velocities without breaking in the slightest degree the evenness of its surface; that, while at high velocities all other formations dashed the water into spray or raised it in waves above the surface, this vessel, at velocities of 16 or 18 miles an hour, appeared to give no motion to any particles of water, excepting such as happened to lie in its path. He considered the entrance into smooth water without ruffling the surface as the criterion of minimum resistance.

Mr. Russell observed, that the form had been constructed on a hypothetical view of the subject, viz. that the minimum force requisite to alter the position of any fluid particle would be that which gave to the particle a uniformly accelerated velocity through the former half of its path, and a uniformly retarded velocity during the remainder; that the well-known relation of the coordinates of the parabola accomplished this in the manner formerly explained, but that he rested for the proof of the correctness of the theory upon the experiments he had already adduced.

Mr. Russell then described a very simple mode of construction, by which the ordinates of a circle or a table of sines might be used so as, in the most elementary mechanical manner, to form a very close approximation to the solid of least resistance; and he concluded by drawing the lines of a vessel of given dimensions according to the new formation of least resistance.

### *On Vibration of Railways. By Capt.* DENHAM, *R.N.*

Capt. Denham ascertained that the vibrating effects of a passing laden railroad train in the open air extended laterally on the same level 1110 feet, (the substratum of the positions being the same,) whilst the vibration was quite exhausted at 100 feet when tested vertically from a tunnel.

The tunnel was through a stratum of sandstone rock: the rails laid in the open air on a substratum of 12 feet of marsh over sandstone rock. The method of testing was by mercury reflecting objects to a sextant. The experiments were made in the neighbourhood of Liverpool.

### *On certain points in the Theory of the Construction of Railroads. By the Rev. Dr.* LARDNER *and* C. VIGNOLLES.

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### *On the Monthly Reports of the Duty of Steam-engines employed in draining the Mines of Cornwall. By* JOHN TAYLOR, *F.R.S., Treasurer of the British Association.*

Mr. Taylor observed that he had found at this and other Meetings of the Association considerable interest to be expressed with regard to this method of recording the actual effect produced by the con-

sumption of a given quantity of fuel, and recommended it to the notice of engineers in general. The monthly reports alluded to gave the means of comparing one engine with another in this district; they also afforded an historical view of the progress of improvement in this important machine; and they had, Mr. Taylor believed, contributed largely to that improvement, by the emulation and attention excited by them in the persons who had the charge of constructing and managing the engines.

Mr. Taylor stated that the work done in the best engines now employed in Cornwall by the consumption of one bushel of coal, required ten or twelve years ago the consumption of two bushels; that during the period of Boulton and Watt's patent four bushels were consumed to do the same work, and that in the earlier stages of the employment of steam power the quantity of coal used was 16 bushels. So that by the progressive advance of improvement one bushel had become sufficient for the duty that formerly required sixteen.

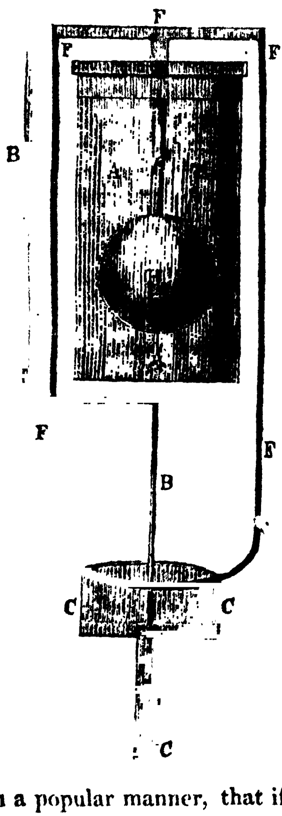
Mr. Taylor, in remarking on the importance of this subject to the deep mines of Cornwall, stated, that the steam-engines now at work for the purpose of draining the mines there were equal in power to at least 44,000 horses, and that as some doubts had frequently been expressed as to the accuracy of the results shown by the duty reports, he had compared them some time since with the accounts of the coal actually used in some of the principal mines at different periods, by which he found the saving of money was as great as the reports indicated, and that their general accuracy was borne out fully by the account books, where this was incontestably proved.

## *Description of a Self-registering Barometer. By Professor* STEVELLY.

During the oscillations of the common barometer, when it falls, a certain quantity of mercury is added to that already in the cistern, which of course adds so much to its weight; on the contrary, when it rises, mercury retires from the cistern, which thereby becomes so much lighter than before. If, then, the tube of a barometer be fixed firmly in its place, but the cistern be by any means so suspended as to move downwards by equal distances for equal additions to its weight, and to rise similarly for similar diminutions of its weight, it is clear that a scale may be placed beside the cistern; and an index carried by the cistern may be made to mark upon the scale a variety of positions corresponding to the rising and falling of the common barometer. It may be shown to any person even slightly conversant with mathematical subjects, that the range of this scale may be made to bear any proportion to that of the common barometer. Supposing for an instant what is now stated to be accomplished, it is obvious that a pencil may be so attached to the cistern as to rise and fall with it, and thus to mark on a properly ruled sheet of paper, carried by clockwork across the instrument, the indications

of the barometer at the successive hours of the day; and thus a curve representing the actual diurnal oscillations of the barometer can be placed before the eye, and a registry kept from day to day on separate sheets of paper. The mean curve can also be had by making the pencil traverse, day after day, for a long period, the same sheet of paper; for the pencil-marks will at length become blackest and heaviest upon the parts corresponding to the mean curve; and thus all the labour of actual observation, registry, &c. will be avoided, and thus, too, much of the trouble of reduction, if not all, will be saved.

Many mechanical methods of suspending the cistern will readily suggest themselves to persons conversant with practical matters; but the method that is preferred by the author is by a mercurial hydrometer, the cistern, for the sake of stability, being suspended underneath the hydrometer, as in Ronchetti's modification of Nicholson's hydrometer. The accompanying drawing will give an idea of the form of the instrument; the following is the description of it. The guide-wheels and supports are omitted.




B the barometer tube (it may be of iron) firmly fixed in its place, and dipping below into

C, the cistern, which is suspended by

F, a frame, supported by

S, the pillar or stem of

H, the hydrometer ball, which floats in

A, a vessel firmly fixed, and containing the mercury (or other fluid) in which the hydrometer floats.

In the description of this instrument given to the Subsection, it was supposed that the surface of the mercury in the cistern and in the vessel A were so large that the rising or falling of the fluid in these vessels might be neglected; also, since the instrument is very sensible, it was supposed that the lower part of the barometer tube which dips into the cistern, should be rendered very small, in order to diminish unsteady oscillation. Also the internal part of the barometer tube B at the upper part, the external part where it dips into the mercury in the cistern, as well as the cistern and the vessel A at the surfaces of the mercury in them, were all supposed to be cylindrical. And it was then shown in a popular manner, that if the internal cross section of the baro-

meter tube at its upper part were made equal to the cross section of the pillar or stem of the hydrometer, the sensibility of the instrument would be too great for practice; the scale in that case would be lengthened out indefinitely, since the hydrometer could never sink sufficiently to attain a position of equilibrium upon a fall of the barometer, and *vice versâ.* But if the cross section of the stem or pillar of the hydrometer be made twice as great as the internal cross section of the upper part of the barometer, the rising and falling of the cistern would be exactly equal to the rising and falling of the common barometer; and therefore the scale of this instrument would then be equal to the scale of the common barometer; and between these limits any desired scale, however long, may be obtained. A scale shorter than that of the common barometer may also be had by increasing the cross section of the stem of the hydrometer beyond the above limit; but this is not likely to be ever desired. When it is desirable to save expense, the hydrometer may be made to float in water; but of course its dimensions will require to be much greater in that case: or the cistern may be counterpoised, and a cylinder like the stem of the hydrometer, dipping into the mercury, may, by its varying buoyancy, be made to restore the equilibrium.

The exact mathematical formula which gives the relation of the scale to that of the common barometer, whatever be the dimensions of the parts of the instrument, is of the form $\delta h = \delta h' \times C$, where $\delta h$ is the variation of the height of the common barometer, $\delta h'$ is the corresponding part of the scale of this instrument, and C a constant depending for its value upon the dimensions of the several parts of the instrument.

Professor Stevelly also described a very simple and cheap instrument for weighing hydrometrically, the sensibility of which is very remarkable,—a hydrometer-ball with a stem of steel wire, having upon it one or two dots of gold, and a scale-pan attached to it, either above as in Nicholson's, or below as in Ronchetti's modification of the hydrometer. An index, or a microscope with a horizontal wire, is attached to the side or cover of the vessel in which the hydrometer floats in such a way that it may be steadily and slowly raised or lowered to mark the position of the gold-dot, instead of taking the indications from the surface of the fluid, as in the common method. The weight of the substance to be weighed is then had by placing it in the scale-pan, bringing the index or wire of the microscope to mark the position of the gold-dot, then removing the substance and substituting for it known weights until the dot is again brought to the same position. Since the adjustment takes place at the instant of using the instrument, it becomes almost incapable of being deranged, and thus a very correct balance may be had by a common apothecary's phial, with a little mercury to steady it, and a knitting-needle pushed down through its cork, and a scale-pan placed above. Every person knows the difficulty of adjusting the common hydrometer, and its liability to derangement.

The same principle may be readily conceived to apply to the construction of a self-registering rain-gauge.

Mr. Andrew Pritchard exhibited examples of various kinds of apparatus constructed by him for illustrating the Polarization of Light; and gave a brief account of his improved achromatic microscope, one of which was placed upon the table.

The construction of a simple polariscope invented by Mr. Pritchard was explained. The crystals to be examined were mounted in slides and introduced between tourmalines, by which means sections of any crystals that present themselves may be examined, and the cell of the upper tourmaline being removeable can be employed for other experiments. A lens was attached for condensing artificial light.

The mechanical part of the achromatic microscope produced was constructed on the principles recently published by Dr. Goring and Mr. Pritchard in their works on the microscope: the chief feature in the optical part was the execution of a set of object-glasses which admitted a pencil of light of *sixty-eight* degrees, free from spherical and chromatic aberration, having the oblique pencils nearly correct and the field of view moderately flat. Mr. Pritchard stated expressly of this instrument, that it was the simplest that had yet been constructed that would accomplish all the work that might be required of a microscope, either for general examination, dissection, or minute investigation.

Preparations of various classes of microscopic objects in Canada balsam were exhibited.

Mr. Hawkins explained the principle of Saxton's locomotive Differential Pulley; and a mode of producing rapid and uninterrupted travelling by means of a succession of such pulleys set in motion by steam-engines or by horses.

Mr. Cheverton read a paper on Mechanical Sculpture, or the production of busts and other works of art by machinery, and illustrated the subject by specimens of busts and a statue in ivory, which were laid on the table. This machine, in common with many others, produces its results only through the medium of a model to govern its movements; but it has this peculiarity, that the copy which it makes of the original is of a size reduced in any proportion, and that it is enabled to effect this result not merely on surfaces, such as bas reliefs, but in the round figures, such as busts and statues.

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Mr. Ettrick gave an account of certain improvements proposed by him in the Astronomical Clock, giving the pendulum a free motion at right angles to the line of its motion, and thereby preventing the tendency to acquire a circular motion by any improper adjustment of the pendulum-spring.

He described a mariner's steering-compass provided with two adjustments, whereby the card was made to point *true* bearings on the horizon; the variation and local attraction being allowed for by regulating the position of the needle on the card.

He also read an account of certain improvements on steam-engines, for making available the power of the steam of high-pressure boilers, which is below the pressure of the atmosphere, by allowing the high-pressure steam to pass off into the atmosphere, and allowing the steam of low pressure to pass into a condenser through a secondary slide. He gave a description of a method of securing the seams of boilers by longitudinal instead of the present circular clenches; and of a machine for drilling boiler-plates as rapidly as they can be punched by the punching-machine.

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Mr. Roberts exhibited a machine which renders objects visible while revolving 200,000 times a minute.

If a firebrand be whirled, in the dark, round a centre in a plane perpendicular to the eye of the spectator, it will present the appearance of a luminous circle. From this fact it has been inferred, that the impression on the retina made by the luminous body in its passage through every point of the circle, remains until the body has completed a revolution. How rapidly soever the firebrand may be made to revolve, the circle, and therefore every part of it, will be distinctly visible: hence a probability arises, that at the greatest attainable velocity a perfect impression of the object in motion will still be produced on the optic nerve, provided that the time of viewing such object be limited to that which is required for passing through a small space—small, at least, with reference to the size of the revolving body,—and also that no other object be presented on the field of vision before the former spectrum shall have vanished from the eye; unless in the case of the same object under similar circumstances. The former of these conditions is provided for in machine No. 1, in which the eye-hole is made to travel through 180 feet between every two inspections of the moving object, and which object is made to assume a different position at each successive inspection. The latter condition is included in machine No. 2; the object is there presented to the eye in one position only.

## STATISTICS.

*On the Statistics of the Dublin Foundling Hospital, and on Child Desertion in the City of Dublin.* By Henry Maunsell, *M.D., Professor of Midwifery to the Royal College of Surgeons in Ireland, and Ordinary Member of the Medical Society of Leipzig.*

During thirty-four years, comprised between 1798 and 1831 (inclusive), there were admitted 51,523 children. Of these, 700 were immediately restored to their parents, and 12,153 died immediately in the nursery. These latter must be considered as having been destroyed by the act of desertion, and therefore cannot fairly be charged to the account of the hospital.

| | | |
|---|---|---|
| Let us deduct then from the original admissions, viz.... | | 51,52 |
| Died immediately........................ | 12,153 | 12,85 |
| Returned immediately .................... | 700 | |
| The remainder sent to nurse will be .............. | | 38,67 |
| Of these there are now alive in the country, under their ninth year ................................ | | 95 |
| Balance to be accounted for ...................... | | 37,71 |

Let us now examine how many of these have reached their nint year. 16,976 deaths under the ninth year have been ascertained t have taken place in the country; and 8278 children were lost sigh of in different years (from the first to the fourteenth) of their age In the records of the hospital, those children whose fate has no been reported are considered as dead; but as it is certain that chil dren were frequently retained by their nurses from motives of af fection, it becomes necessary to inquire what proportion those s retained bear to the whole. It appears that 6949 were never brough back to the hospital after having been first sent to nurse; and it i probable that all these died so early as to make it not worth th nurse's while to apply for the small sum of wages to which she woul be entitled. The number unreported is at its minimum in the thir year of the child's age; and from that period it increases for seve ral years. The fourth or fifth year being the period when the nurs *might become fearful of the child's being removed from her*, w shall probably not be far wrong in leaving out of the question thos that *may* have been kept back during the first three years, and i supposing that all those unreported after that age have been re tained from affection.

Of the whole balance of 37,717, we shall then be able to accoun for, as alive at the ninth year, the following:

| | |
|---|---|
| Drafted into the hospital above their ninth year ........ | 994 |
| Left in the country after their ninth year............... | 147 |
| Restored to parents, having been returned from nurse ... | 35 |
| Probably retained from affection ...................... | 105 |
| Total living at their ninth year.............. | 12,83 |

During twenty-four years of [illegible] period included in the [illegible]regoing statement, children were received at all seasons of the [illegible]ear, and without any restriction whatsoever; [illegible] by carriage from distant places in sever[illegible] admissions were restricted, during the winter months, [illegible] deserted in the city of Dublin and its environs; and no child wa received without a certificate of its actual desertion, and of its pa rents being unknown. Five pounds were also required to be for warded with each child from its parish. In consequence of these

changes, the average annual admissions at once fell from 2000 to 500; and the rate of mortality was altered as will appear in the following statement:

| | |
|---|---|
| During the five years from 1822 to 1826 (inclusive) there were admitted | 2339 |
| Returned to parents immediately | 14 |
| Balance to be accounted for | 2325 |

Of these,

| | | |
|---|---|---|
| Died immediately | 172 | |
| Died at nurse before ninth year | 819 | |
| Died after return from nurse | 39 | |
| Not reported up to ninth year, many of whom may have been retained from affection | 131 | |
| Total dead or unaccounted for | | 1161 |
| Total alive at ninth year | | 1164 |

According to the Northampton table, the survivors at the ninth year, of 2325, should be only 1143.

Since the year 1831, the Foundling Hospital has been closed, and each parish has been charged with the care of its own foundlings.

The accompanying table will show the mortality among foundlings in those parishes of the city of Dublin in which records were kept.

Return of Mortality of Foundlings in some Parishes of Dublin, from the closure of the Foundling Hospital in 1831 to January 1835.

| | Anne's. | Bride's. | George's. | Mary's. | Peter's. | Thomas's. |
|---|---|---|---|---|---|---|
| Desertions | 18 | 32 | 20 | 26 | 74 | 37 |
| Died | 11 | 12 | 14 | 10 | 21 | 24 |

The following table shows the number of infanticides that have been ascertained to have occurred in Ireland during two years preceding 1831, in the course of which the hospital was closed, and also during the years 1831, 1832, 1833, and 1834. Owing to the imperfection of the records, the number in all those years must be greatly below the truth. It is however so far valuable as it shows a contrast between periods during which the Foundling Hospital was closed and

### INFANTICIDES.

| | 1829. | | 1830. | | 1831. | | 1832. | | 1833. | | 1834. | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Inquest held. | Inquest not held. | Inquest held. | Inquest not held. | Inquest held. | Inquest not held. | Inquest held. | Inquest not held. | Inquest held. | Inquest not held. | Inquest held. | Inquest not held. |
| Leinster.......... | 16 | ... | 14 | ... | 29 | ... | 35 | ... | 40 | ... | 31 | 1 |
| Ulster............. | 22 | ... | 12 | ... | 13 | ... | 20 | ... | 26 | ... | 29 | 1 |
| Connaught....... | 2 | ... | 2 | ... | 2 | ... | 1 | ... | 4 | ... | 3 | ... |
| Munster.......... | 18 | ... | 14 | 2 | 22 | ... | 23 | ... | 30 | ... | 24 | 5 |
| Total...... | 58 | ... | 42 | 2 | 66 | ... | 79 | ... | 100 | ... | 87 | 7 |

The average expense of each child in charge of the Foundling Hospital, at a time when a large boarding-school establishment, with educational and hospital staff, was kept up, may be stated at *5l.* per annum. In the year 1826 the whole number of children amounted to 6339; and of these upwards of one sixth were maintained in the hospital at an annual expense, for feeding and clothing, of from *6l.* to *7l.* per child. The whole expense during that year was 33,729*l.* 9*s.* 10½*d.*, being a fraction more than *5l.* per child. This included all expenses, salaries, wages, annuities, repairs of buildings, and apprentice fees.

Under the present parochial system, the nurses' wages alone varies from 1*s.* 6*d.* to 2*s.* per week, or from 3*l.* 18*s.* to 5*l.* 4*s.* per annum. There is also an additional expense incurred with each child for keeping before it is sent to nurse, which in one parish (Anne's) amounts to 5*s.* per diem. The parish children are as yet too young to require any additional expenditure; but in a few years a considerable demand must be made for education, clothing, and apprentice fees.

The items of expenditure in the Foundling Hospital, exclusive of salaries and expenses of establishment, are as follow:

Allowance to nurse for first five years, 3*l.* per annum, with, at the end of the first year, a gratuity for good nursing, of 2*l.*; after the fifth year, 10*s.* is allowed yearly for clothing and education.

A table has been constructed, showing the proportion of the sexes of children received into the Foundling Hospital during twenty-four years; from which it appears that of 32,324, 15,179 were males, and 17,145 females.

The large table from which the statements made in the commencement of this abstract were deduced, contains several particulars respecting the children in charge of the hospital since 1798. These, however, would not admit of being presented in an abstract.

Tables have been constructed, showing the yearly number of admissions into the Foundling Hospital from the different parishes of Dublin during twelve years preceding its closure, and also the yearly number deserted in the same parishes since the shutting up of the hospital in 1831. The results of these are contrasted in the accompanying tables, which show the *average* yearly desertions under each system.

TABLE showing the average Yearly Desertions in each Parish of Dublin during the Years 1827, 1828, 1829, and 1830.

| Andrew's. | Anne's. | Audeon's. | Bride's. | Catharine's. | George's. | James's. | John's. | Luke's. | Mary's. | Mark's. | Michael's. | Michau's. | Nicholas Within. | Nicholas Without. | Paul's. | Peter's. | Thomas's. | Werburgh's. |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 9 | 3 | 7 | [illegible] | 12 | 12 | 5 | 2 | $\frac{1}{4}$ | 29 | 5 | $\frac{1}{2}$ | 15 | $\frac{1}{4}$ | $1\frac{1}{2}$ | $\frac{1}{4}$ | 30 | 15 | $1\frac{1}{2}$ |

TABLE showing the average Yearly Desertions in the same Parishes since closure of the Foundling Hospital, comprising part of 1831, 1832, 1833, and 1834; calculated from Returns of Numbers and Assessments.

| Andrew's. | Anne's. | Audeon's. | Bride's. | Catharine's. | George's. | James's. | John's. | Luke's. | Mary's. | Mark's. | Michael's. | Michau's. | Nicholas Within. | Nicholas Without. | Paul's. | Peter's. | Thomas's. | Werburgh's. |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| *6 | 5 | No Accounts. | 8 | *14 | 5 | †6 | No Accounts. | $\frac{1}{4}$ | †20 | No Accounts. | *4 | All refused. | ‡$\frac{1}{4}$ | No Accounts. | *8 | 20 | †12 | †$2\frac{1}{2}$ |

* Thus marked, calculated from assessments.
† Thus marked, had not accounts for the whole period.
‡ No accounts in Nicholas Within; but as there have been some, the average of the four years cannot be less than $\frac{1}{4}$.

### *On Wages in India. By Lieut.-Colonel* SYKES.

Colonel SYKES read a statement of the rate of wages in India measured in kind, and also measured in money. He enumerated some of the various places in which he made his inquiries, for the purpose of showing that towns and villages the most distant from each other were chosen to prevent the mistake of adopting local rates as if they were of general operation. Labourers in India are seldom paid in money, except when grain is very dear, a custom obviously injurious to the labourers. Wages in India are very low. When paid in money, three rupees (rather less than six shillings) is the usual monthly pay of a labourer in agriculture, without food, clothes, lodging, or any other advantages. The cause of the low rate of wages of labourers in India appears to be the small quantity of useful work they do. The author states, that when in the Poona collectorate on the 16th of February 1829, he overtook twelve or fourteen men and women with bundles of wheat in the straw on their heads. On inquiry, he found they had been employed as labourers in *pulling up* a field of wheat. Their wages had been five sheaves for every hundred gathered: two or three of the men had got five sheaves each, the majority only four, and none of the women more than three. Five sheaves, they said, would yield about an imperial gallon of wheat, and would sell for about threepence-halfpenny sterling.

At the end of his paper the author formed some tables, in which he placed in juxtaposition the rates of wages paid to different classes of artificers, servants, and labourers under the British government in 1828, and Peshwa's government in 1814; and also the prices of grains, pulses, and other articles of the ordinary consumption of labourers under the British government and under the Peshwa's government at the same periods, viz. 1814 and 1828.

These tables show a marked improvement in the wages of all classes of labourers, although grain became from 20 to 50 per cent. cheaper under the British than under the Peshwa government. This increase has been greatest in the wages of the lowest classes of labourers.

### *Remarks on the Statistics contained in the Ordnance Survey of the Parish of Templemore. By* C. BABBAGE, *F.R.S.*, &c.

To discover those principles which will enable the greatest number of people by their combined exertions to exist in a state of physical comfort and of moral and intellectual happiness, is the legitimate object of statistical science.

To effect this object, religious, moral, and practical instruction are necessary; and to enable us to reason from the sound basis of experience, we must patiently collect and classify those facts which affect the well-being of mankind. These facts have been so collected for one district of Ireland on an extensive scale in the present

volume, and so well arranged and classified, that each person desiring to put his theoretical views to the test of experience, or to generalize from the fertile field of instances here collected, may most readily separate the facts which he desires to make use of. Professor Babbage concluded with some observations on the manner in which, in future periods, this monument of industry and intelligence will contribute to the amelioration of the moral and physical condition of the people.

*Inquiries carried on by the Statistical Society of Manchester.*

Mr. W. R. Greg and Mr. W. Langton presented on behalf of the Statistical Society of Manchester the heads of inquiry and the forms used in conducting their inquiries into the state of education, into the condition of the working classes, and into the means existing for the religious instruction of the working classes.

These inquiries have been instituted in Manchester and in some of the neighbouring towns.

Mr. W. Langton read to the Section an abstract of the Report of a Committee of the Manchester Statistical Society on the State of Education in the Borough of Manchester in 1834.

The Report showed the following results.

The numbers then attending the different schools in Manchester were 43,304; of whom

| | |
|---|---|
| 10,108 | attended day and evening schools *only*, |
| 10,011 | attended both day and Sunday schools, |
| 23,185 | attended Sunday schools *only*. |
| 43,304 | |

The population of the borough of Manchester being then about 200,000, the number of persons receiving instruction was $21\frac{1}{4}$ per cent. of the population. Of those attending day and evening schools the numbers gave a proportion of about 10 per cent. of the population.

From the number of about 43,000 scholars 10,000 were deducted as being under 5 and above 15 years of age, which left about 33,000 as the number of children between the ages of 5 and 15 under course of instruction. The whole number of children between the ages of 5 and 15 in the borough of Manchester being estimated at 50,000 or $\frac{1}{4}$th of the whole population, it would thus appear that about $\frac{2}{3}$rds of the number are educated, and that $\frac{1}{3}$rd are receiving no instruction whatever.

The returns made by the overseers to Government under Lord Kerry's motion, had been examined by the committee in three townships out of nine which constitute the borough of Manchester, and considerable errors had been discovered in each return.

In the township of Manchester alone, which contained a popula-

tion of 142,000 in 1831, there were entirely omitted in those returns 1 infant school, 10 Sunday schools, and 176 day schools, which existed at the period when those returns were made, and contained 10,611 scholars. False returns were made by one individual of three Sunday schools that never existed at all, and which were stated to contain 1590 scholars; and double returns were made of three other schools, containing 375 scholars; so that the total error in those returns for the township of Manchester alone was 181 schools and 8616 scholars. Besides this, eight dame schools were reported as infant schools.

The tables annexed to the report of the committee gave a classification of the schools in the borough, showing the relative number of scholars of each sex, and the date of their establishment; the mode in which supported; the terms of payment in the dame schools, common day schools, evening and infant schools; the course and mode of instruction pursued; the country and religious profession of the masters or mistresses; the number of years they had been engaged in teaching, and the number who had been educated for the employments, &c.

Upon the superior private and boarding schools no minute report was given. One of the Mechanics' Institutions was stated to be in a very prosperous condition.

The education given in the common day schools, containing nearly 7000 children, was represented to be generally very poor, few of the teachers being at all qualified for their task; and the committee consider that no effectual means can be taken to render these schools efficient until proper seminaries are established for the instruction of the teachers themselves.

The report states that of 4722 children attending the dame schools, the vast majority receive no instruction at all deserving of the name, and derive little benefit from their attendance at school but that of being kept out of harm during a few hours of the day. The establishment of infant schools on a large scale is recommended with a view gradually to supplant the dame schools.

The Sunday schools which had above 33,000 scholars on their books, with an average attendance of nearly 25,000, are classed in various ways in the tables. They are reported by the committee to form a most important feature among the means existing for the education of the lower classes of the people, and their influence is represented as highly beneficial.

Mr. Greg and Mr. Langton also presented to the Section an unpublished Table showing a general summary of schools in Bury, Lancashire; drawn up from an investigation just completed in that borough by the Education Committee of the Manchester Statistical Society.

GENERAL SUMMARY OF SCHOOLS AND SCHOLARS IN THE BOROUGH OF BURY, LANCASHIRE, IN JULY 1835.

| POPULATION ESTIMATED TO BE 20,000. | SCHOOLS. | Scholars. Age. Under 5. | Age. Between 5 and 15. | Age. Above 15. | Sex. Male. | Sex. Female. | Total. | Per Centage. To the total population, estimated at 20,000. | Of the whole number of Scholars. | Of the whole number of Sunday Scholars. |
|---|---|---|---|---|---|---|---|---|---|---|
| Sunday Schools. Church of England... Connected with Churches... | 2 | 122 | 582 | 309 | 456 | 557 | 1013 | 5.06 | 17.69 | 23.98 |
| Sunday Schools. Church of England... Connected with Factories... | 3 | 47 | [illegible] | 74 | 210 | 232 | 522 | 2.61 | 9.11 | 12.35 |
| Sunday Schools. Roman Catholic | 1 | 20 | [illegible] | 20 | 75 | 80 | 155 | .78 | 2.71 | 3.67 |
| Sunday Schools. Dissenters | 6 | 67 | 2121 | 346 | 1128 | 1406 | 2534 | 12.67 | 44.25 | 60.00 |
| Total | 12 | 256 | 3219 | 749 | 1599 | 2325 | 4224 | 21.12 | 73.76 | 100.00 |
| | | 6.06 pc | 76.21 pc | 17.73 pc | [illegible] pc | 55.04 pc | | | | |
| Returned also as Day or Evening Scholars | | | | | | | 1122 | 5.61 | 19.60 | |
| Receiving Sunday School tuition *only*... | | | | | | | 3102 | 15.51 | 54.16 | |
| Day Schools. Supported by Parents. Dame Schools | 29 | 190 | 650 | ... | 258 | 582 | 840 | 4.20 | 14.67 | |
| Day Schools. Supported by Parents. Common Boys' and Girls' Schools | 17 | 79 | 717 | 12 | 535 | 273 | [illegible] | [illegible] | 14.11 | |
| Day Schools. Supported by Parents. Superior Private and Boarding Schools | 8 | 6 | 156 | 12 | 51 | 123 | 174 | .8[illegible] | 3.03 | |
| | 54 | 275 | 1523 | 24 | 844 | 978 | 1822 | 9.11 | 31.81 | |
| Day Schools. Assisted by Charity... Infant Schools | 2 | 192 | 51 | ... | 137 | 106 | 213 | 1.22 | [illegible] | |
| Day Schools. Assisted by Charity... School at Hudcar Works | 1 | 21 | 20 | ... | 14 | 27 | 41 | .20 | | |
| Day Schools. Entirely Free... School in Bolton Street | 1 | ... | 10 | ... | ... | 10 | 10 | .05 | 6.43 | |
| Day Schools. Entirely Free... National School | 1 | ... | 280 | ... | 180 | 100 | 280 | 1.40 | | |
| Day Schools. Entirely Free... Free Grammar School | 1 | ... | 70 | 8 | 78 | ... | 78 | .39 | | |
| Total | 60 | 488 | 1954 | 32 | 1253 | 1221 | 2474 | 12.37 | 43.20 | |
| | | 19.73 pc | 78.98 pc | 1.29 pc | 50.65 pc | 49.35 pc | | | | |
| Evening Schools, exclusive of those attached to Sunday Schools | 6 | ... | 52 | 99 | 100 | 51 | 151 | .75 | 2.64 | |
| | | | 34.44 pc | 65.56 pc | 66¼ pc | 33¾ pc | | | | |
| Total Number of Schools and Scholars | 78 | ... | ... | ... | ... | ... | 5727 | 28.63 | 100.00 | |
| Evening Schools attached to Sunday Schools | 7 | ... | 211 | 174 | 192 | 193 | 385 | 1.92 | 6.72 | |
| | | | 54.8 pc | 45.2 pc | 49.87 pc | 50.13 pc | | | | |
| Average attendance at Sunday Schools | | ... | ... | ... | ... | ... | 3568 | | | |

| |
|---|
| Street. |
| Number. |
| Name of Father of Family or Chief Lodger. |
| Number of Family. |
| House, Room, Cellar, or Lodgings. |
| Number of Children above 12. |
| Number of Children under 12. |
| Number of Children receiving Wages. |
| Employed in Cotton Factories. Spinners, Carders, Stretchers, Piecers, Reelers, Powerloom Weavers, Dressers, F.C.H. |
| Weavers. Fancy, Silk, Cotton, Flannel, Blanket, Woollen Cloth. |
| Warehousemen. Winders and Warpers, Clerks, Salesmen, Travellers, Porters & Packers, Errand-boys, &c. |
| Building Trades. Bricklayers, Stonemasons, Joiners and Cabinet-makers, Plumbers and Glaziers, Labourers, Blacksmiths. |
| Other Occupations. Carters, Calenderers, Dyers, Fustian Cutters, Shoemakers, Tailors, Sempstresses, Washerwomen, Painters and Plasterers, &c. |
| Religious Profession. Church, Dissenter, Roman Catholic, Jew, None. |
| Number of Family in Benefit-Club, Building-Club, Secret Order, or Clothiers' Club. |
| Weekly Rent of Dwelling. |
| Number who can read. |
| Number who can write. |
| Is the House well furnished? |
| Is the House comfortable? |
| How many Beds are there for the Family. |
| Country of Parents. English, Irish, Scotch, Welch, Foreigners. |

*List of Inquiries adopted by the Statistical S[illegible]iety of Manchester concerning the Means existing for the R[illegible]us Instruction of the Working Classes in Manchester.*

1. How many sittings are there of all [illegible]scriptions in your church or chapel, distinguishing those which a[illegible] *free* from those which are *not free*?

2. How many services have you o[illegible]ach Sunday, and what is the average attendance at each, in the f[illegible]ittings and in the appropriated sittings?

3. Of the sittings which are n[illegible]ree, how many are at the rate of *5l.* per annum each and under, [illegible] how many of them are let?

4. What accommodation hav[illegible]u for your Sunday scholars in your church or chapel? Wh[illegible] their number and average attendance at each service?

5. Is any district of the to[illegible]igned to or adopted by you as the sphere of your personal [illegible] among the poor, and what is that district?

6. Have you conne[illegible]ith your church or chapel any society for the purpose of as[illegible]g your labours among the poor? and if so, what is the nature [illegible]xtent of that society?

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The Rev. E. S[illegible]EY presented the results of a Statistical Inquiry into the educati[illegible]and religious state of the parish of Alderley in Cheshire, which[illegible] been drawn up for the Statistical Society of Manchester in[illegible]

*On the Glasgo[illegible]ewell or House of Correction.* *By* Dr. CLELAND.

The followi[illegible]bjects are discussed :—

The constru[illegible] of the building; the number of prisoners on the 23rd July 1835[illegible]eir religious persuasion, age, and sex; abstracts of accounts; det[illegible] o[illegible] salaries and wages, and diet; proportions of re-committals (males, [illegible]emales, and terms of first sentence distinguished). The follo[illegible]ng [illegible]a General Abstract for ten years.

| Year. | Total Committal. | [illegible]aily [illegible]erage. | Cost to the Public of maintaining each Individual. £ | s. | d. | Cost of the Establishment to the Public. £ | s. | d. |
|---|---|---|---|---|---|---|---|---|
| 825. | 1540 | 200 | 2 | [illegible] | 9 | [illegible]593 | 0 | 0 |
| 826. | 1398 | 250 | [illegible] | [illegible]4 | 9 | 934 | 13 | 8 |
| 827. | 1696 | 257 | 3 | 3 | 3 | 813 | 8 | 11 |
| 828. | 1770 | 287 | 2 | 1 | 8 | 598 | 13 | 6 |
| [illegible] | 1721 | 27[illegible] | 3 | 2 | [illegible] | 858 | 2 | 6 |
| 83[illegible] | 1961 | 29[illegible] | 3 | [illegible] | 6½ | 945 | 17 | 4 |
| 831. | 1905 | 2[illegible]1 | 3 | 3 | 3 | 920 | 10 | 2 |
| 832. | 1953 | [illegible]02 | 2 | 12 | 9 | 796 | 3 | 5 |
| 833. | 2075 | [illegible]27 | 2 | 4 | 3 | 725 | 18 | 7 |
| 834. | 19[illegible] | 320 | 1 | 16 | 11 | 590 | 10 | 0 |

The employments of the prisoners are specified; the medical attendance exemplified; and the author adds his decided testimony to the advantages resulting from the introduction of solitary confinement.

*On the Causes which affect the Proportions between the Numbers of Accusations and Convictions in the Metropolitan District, and on the Effects which well-managed Houses of Correction have in repressing Crime.* By E. Ha[illegible]well.

*On the Punishment of Death in Prussia, Norway, and Brunswick.* By Mr. Fox.

Extract of a letter received by the Committee of the Capital Punishment Society, dated Berlin, March 10, 1835.

The entire Number of Executions in each Year.

| Year. | Arson. | "Voluntary Manslaughter." | Murder. | Total. |
|---|---|---|---|---|
| 1818. | 1 | 3 | [illegible] | 9 |
| 1819. | — | 2 | [illegible] | 8 |
| 1820. | — | [illegible] | [illegible] | 13 |
| 1821. | — | — | 14 | 14 |
| 1822. | — | 1 | 4 | 5 |
| 1823. | — | 4 | 6 | 10 |
| 1824. | — | 2 | 10 | 12 |
| | | In five years for | murder....47 | |
| 1825. | — | 1 | 3 | 4 |
| 1826. | — | 1 | 4 | 5 |
| 1827. | — | 2 | 5 | 7 |
| 1828. | — | 2 | 10 | 12 |
| 1829. | — | 1 | 4 | 5 |
| | | In five years for | murder....26 | |
| 1830. | — | — | 4 | 4 |
| 1831. | — | 1 | 8 | 9 |
| 1832. | — | — | — | 2 |
| 1833. | — | — | 2 | 2 |
| 1834. | — | — | 2 | 2 |
| | | In five years for | murder....16* | |
| | 1 | 22 | 100 | 123 |

* In the last three years 22 were sentenced to death for murder, of whom only 4 were executed, [the remainder being imprisoned at hard labour.]

### Proportions of Convictions and Executions.

MURDER.

5 years ending 1824, capitally convicted, [illegible]9, executed 47 :—or $\frac{68}{100}$
5 years...... 1829,.................. 50,........ 26 :—or $\frac{51}{100}$
5 years...... 1834,.................. 43,........ 16 :—or $\frac{37}{100}$

Here there is a diminution of executions in each of the two last periods, and at the same time a diminution of crime. If we compare the two extreme periods, we find one third *less* crime in the last with 16 executions, than in the first with 47 executions.

The mean population of Prussia during the same period may be taken at 12,303,535, that being the amount according to the official census in 1826, which year falls exactly in the middle of the same series of years.

### *On the Social Statistics of the Netherlands.* *By* W. R. Greg.

The essay, drawn up principally from the works of M. Quetelet, has been printed by the author. The information contained in it is classed under the heads of: 1, density of population; 2, number of children to a marriage; 3, education; 4, crimes against person; 5, crimes against property; 6, crimes of great violence. These subjects are discussed in tables for the different provinces of Belgium and Holland, and illustrated by six maps, in which the premises are shaded and numbered in the order of their numerical relations to the above six subjects. Comparisons are occasionally introduced between these results and those attained in France and England.

### *Account of the Normal School in Dublin established by the Commissioners of Education.* *By* Dr. Dickenson.

### *On Cooperative Sh[illegible] f[illegible] the purpose of supplying Workmen with the Necessari[illegible] of [illegible]fe.* *By* C. Babbage, *F.R.S., &c.*

Mr. Babbage gave an account of an instance in which such shops were in operation from [illegible]18 to 1832. He produced tables showing the nu[illegible]r of purch[illegible]s, the quantities of goods sold, and the rate of profi[illegible] every ye[illegible] during that period, and explained the causes which l[illegible] the fail[illegible] and abandonment of the system at the end [illegible] the la[illegible]r year.

### *On [illegible] of Education [illegible] the Deccan* [illegible] *Lieut.-Col.* Sykes, *F. R. S.*

Statistica[illegible] documents relating to the New Colony of Australia were presented by Col. [illegible]

*On the Extension of the Study of Physics.* By Dr. D. B. Reid.

Dr. Reid stated that the importance of a practical knowledge of physical science to a great number of individuals who cannot afford the time or means required for enabling them to attend the courses at present given to professional persons, led him, some years ago, to pay great attention to the simplifying of apparatus, and the introduction of a course of chemistry. The first branch he had paid more especial attention to, which might be accessible to all classes of society. Last winter he completed his arrangements for this purpose, and gave two courses, to put his system to the test of experience. In one, 100 mechanics operated at the same time on twelve different occasions; and in the other, a course was given to 40 young persons, where the same system was adopted. In the mechanics' class, the students were arranged along five boards, each being provided with twenty gas-lamps, one of which was placed alternately on either side. Every pupil received a blowpipe, a test tube, slips of paper on which tests were applied, and also a broad and a narrow slip of glass, such as glaziers throw away. These slips were used for the same purposes as the paper, and also for solution, boiling, evaporation, crystallization, and filtration: the narrow slips, on the other hand, were employed for imitating furnace operations, heat being applied by a common lamp or candle, assisted, where this was necessary, by the blowpipe. The method of using the flat glass for the above operations was illustrated by Dr. Reid, and the specimens handed to the gentlemen attending the section. Experiments conducted in this manner were equally œconomical and effectual in communicating instruction; and the professional student might also, in the same way, repeat again and again, at home, at the most trifling expense, the greater number of those illustrations which he might see in the lecture-room. Dr. Reid stated, that at an expense varying from 2*l.* to 5*l.* every school master might provide himself with an apparatus sufficient to show thousands of experiments on the small scale, and awaken the in[illegible] of his pupils so as to take an interest in science*.

* Since this system was proposed, a number of individuals in London, Edinburgh, and Dublin have stated their intention to introduce it practically both in mechanics' institutions and in schools and academies for the instruction of young persons.

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# INDEX I.

TO

# REPORTS ON THE STATE OF SCIENCE.